Diversion in the Criminal Justice System

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Abstract

This paper provides the first causal estimates on the popular, cost-saving practice of diversion in the criminal justice system. We exploit two natural experiments in Harris County, Texas where first-time felony defendants faced abrupt changes in the probability of diversion. Using administrative data and regression discontinuity methods, we find robust evidence across both experiments that diversion halves reoffending rates (-32 p.p.) and increases quarterly employment rates (+18 p.p.) over 10 years. These improved trajectories persist even 20 years out and are concentrated among young, black men. Further analysis suggests that felony conviction stigma and re-sentencing deterrence are key direct mechanisms.

Keywords: felony records, criminal justice, race, recidivism, labor market

JEL classification codes: J24, K14, K42

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The U.S. criminal justice caseload, whether defined in terms of arrestees, defendants, or detainees, has expanded dramatically over the last half century. As of 2010, more than 8 percent of the adult population in the U.S. had a prior felony conviction, compared with just 3 percent in 1980 (Shannon et al. 2016). As caseload growth has challenged system capacity, legal and procedural strategies have emerged to focus efforts on the most serious offenders. Often this means showing greater leniency to individuals posing minimal risk to conserve resources. One emerging practice is a class of interventions referred to as *diversion* where public officials choose to pause or terminate someone's progression through the justice system. By 2013, thousands of jurisdictions in the U.S. practiced some form of diversion whether at the arrest, prosecution, or institutional stage (Center for Health and Justice 2013). While such programs differ in terms of timing, eligibility, and requirements for successful completion, they share the common goal of limiting a low-risk offender's exposure to the formal justice system.

Despite the growing prevalence of criminal justice diversion programs, little is known about their causal impact on future behavior.[1] Although often motivated as a cost-cutting strategy, diversion likely has profound impacts on the offenders themselves and their communities overall. Recent findings document the damaging effects of criminal convictions and incarceration on employment, education, and reoffending (Dobbie et al. 2018, Stevenson 2018, Mueller-Smith 2015, Aizer and Doyle 2015, Di Tella and Schargrodsky 2013, Raphael 2014, Lovenheim and Owens 2014, Finlay 2009, Pager 2008, 2003). To the extent that diversion buffers individuals from these negative consequences, it may improve their self-sufficiency and minimize further criminal activity. Recent work on the effectiveness of specific deterrence (Bhuller et al. 2016, Hansen 2015, Owens 2009)—the prediction that a past experience of a more severe sanction can decrease the probability of future offending—however, suggests impacts in the opposite direction. These ambiguous theoretical predictions raise the need for convincing empirical evidence.[2]

In this paper, we present the first causal evidence on the effect of diversion in the justice system on recidivism and labor market outcomes. To accomplish this, we use a regression discontinuity (RD) design to evaluate two discrete changes in the use of diversion by criminal courts in Harris County, Texas. The first discontinuity follows a Texas penal code reform in

[1] Chiricos, Barrick, Bales and Bontrager (2007) find that two-year recidivism rates among offenders with a court deferral agreement are significantly lower compared with convicted offenders using data from Florida. While the authors control for offender and county characteristics, estimates may suffer from bias since variation in conviction status is endogenous.

[2] This research is especially urgent given recent instructions by U.S. Attorney General Jeff Sessions to pursue the most serious provable charge for low-risk criminal defendants (Sessions 2017), reversing course for greater criminal justice diversion.

1994 that reduced diversion for offenders charged with certain reclassified drug and property offenses. The second discontinuity follows the unexpected failure of a 2007 jail expansion ballot initiative that resulted in an immediate increase in the diversion rate for low-risk defendants. These policy shifts are attractive from a research perspective because each was implemented rapidly so that defendants charged one day versus the next experienced abruptly different case decisions. In addition, the discontinuities impact a substantial fraction of our sample (between 18 to 24 percent) indicating that the marginal caseload is a non-trivial share of the overall caseload, which improves the external validity of our causal estimates.

Our findings indicate that diversion substantially improves both criminal and employment outcomes over a 10-year follow-up period for first-time felony defendants. The probability of any future conviction declines by 48 percent (-32 percentage points) and the total number of future convictions falls 66 percent (-1.45 new convictions). Quarterly employment rates improve by 53 percent (18 percentage points) and total earnings by 64 percent ($61,540). Notably, the two independent experiments yield estimated treatment effects that are highly consistent, often within several percentage points.

We also demonstrate that the recidivism and labor market impacts persist, if not further expand, up to 20 years following their first felony charge based on results from the 1994 RD. We interpret these findings to indicate that diversion permanently changes an individual's lifetime trajectory, which may relate to the composition of our main study sample. These individuals have been charged with, but not yet convicted of, their first felony offense, which likely is a critical period in their lives.

In the local context, criminal court diversion is most often formalized as a *deferred adjudication of guilt*, which avoids a formal felony conviction and mandates a period of community supervision similar to a probation sentence. On occasion, case dismissal also serves as an extreme form of diversion. In contrast to other potential court interventions like specialized drug or veterans courts, diversion approaches require minimal additional public resources since they rely on established community supervision programs (e.g. probation) or no sanctions at all.

While diversion is often used to reduce the incidence of incarceration, we do not find evidence that changes in incarceration drive our results. In 1994, marginal defendants who were not diverted due to the reform were instead convicted and sentenced to probation. In 2007, we observe significant declines in incarceration sentences for those diverted due to the change in practice, but we do not observe meaningful reductions in actual time served immediately following the focal verdict.

Rather than a change in sanctions per se, we believe that avoiding the lifelong mark of a felony conviction operates as a key causal mechanism in this context. In addition, re-sentencing deterrence for diverted offenses, similar to the mechanism observed in Drago et al. (2009), also contributes to the observed reductions in criminal activity. Finally, the compounding effect of future criminal sanctions due to new criminal activity, which would reinforce and amplify the initial treatment differences, is a factor in the long-term growth in our estimated impacts over such an extended follow-up period.

The largest treatment effects are measured for young, black men with a misdemeanor record—a group that exhibits the highest likelihood of future criminal involvement. Because the criminal justice system disproportionately affects this demographic in the United States (Shannon et al. 2016), expanding the practice of diversion could help reduce the broad socio-economic inequalities recently documented by Chetty et al. (2018).

We do not measure significant discontinuities in observable demographic characteristics, prior criminal histories, or the density of the criminal caseload, allaying concerns regarding endogenous sorting or sample imbalance. We also conduct a number of robustness tests to verify that our findings do not rely on any particular RD specification. Finally, we find no significant effects from placebo tests that apply our RD strategies to the same discontinuity dates in 1993, 1995, 2006, and 2008.

The remainder of the paper is structured as follows: Section 2 describes the two discontinuities in diversion rates in Harris County, Texas that form the basis of our research design; Section 3 addressess the administrative datasets used to perform the analysis; Section 4 outlines our regression discontinuity empirical strategy; Section 5 presents our results and provides evidence supporting our identification assumptions; Section 6 highlights mechanisms that explain our pattern of empirical results; and Section 7 provides concluding remarks. Two appendices (one print, one online) provide results from additional robustness exercises, alternative specifications, and subgroup analyses.

2 Institutional Background

2.1 The 1994 Penal Code Reform

In 1993, the Texas Legislature enacted a reform of its penal code, affecting defendants who had committed their offenses on or after Sept. 1, 1994.[3] The reform sought to "get smart"

[3]Two pieces of legislation accomplished this overhaul: Senate Bills 1067 and 532. In general, prosecutors in Texas are required to file charges within 48 hours of arrest, which limited the opportunity for manipulation

on incarceration through reducing the sanctions for low-risk offenders while increasing time served before parole eligibility for aggravated violent offenders.

A major component of the reform was the creation of a probation-before-incarceration requirement for low-risk, first-time felony offenders. The specific legal wording of this provision disincentivized prosecutors' use of diversion. At issue was the removal of the ability to threaten incarceration to ensure compliance with a diversion agreement, which prosecutors felt severely undermined it's effectiveness. Under the new penal code a second round of probation was required for defendants who violated their diversion agreements since they had not yet been convicted of a felony. The fact that the language of this provision weakened the credibility of diversion was raised by attorneys in October 1993, yet no action was taken to amend the statutory language before the changes took effect (Fabelo 1997).

Not all crimes were impacted by the probated incarceration requirement. The change affected only the lowest felony drug and property crimes, specifically possession of less than 1 gram of a controlled substance[4] and property offenses totaling less than $20,000 in total damages. Diversion rates in the affected caseloads dropped significantly with the new penal code. In lieu of diversion, defendants were typically formally convicted and sentenced to probation as required by statute. Both before and after the Sept. 1, 1994 cutoff, marginal defendants were subjected to community supervision; the main difference from the defendant's perspective was that before the cutoff, one could avoid a felony conviction whereas after a felony conviction was nonnegotiable.

2.2 The 2007 Failed Jail Expansion

During the 2000s, overcrowding in the Harris County Jail was an important concern. This local jail—which houses inmates with shorter sentences and serves several other functions including detention of individuals waiting for trial or to be transferred to the state prison system—had up to 1,900 inmates sleeping on mattresses on the floor by 2005 (Hughes 2005). To address overcrowding, the county sought to expand capacity by 2,500 beds with $195 million to be raised through county bonds for construction of a new jail facility. Before issuing the bonds, the county first sought permission from local voters on the Nov. 6, 2007 ballot.[5]

around the introduction of this penal code reform.

[4]This mainly was comprised of crack cocaine and cocaine as well as heroin, methamphetamine, and other serious drugs. Marijuana possession was covered by a separate part of the penal code and was not impacted by this specific change.

[5]The proposed jail expansion (Proposition 3) was part of a broader bond package being put to local voters in 2007 in response to the county's fast growing population. Together Harris County and the Port of Houston Authority added six local bond propositions to the Nov. 6, 2007 election ballot at combined total of $880

A local campaign against the jail expansion and a large voter turnout led to a narrow and unexpected defeat of the initiative by a vote of 50.6 to 49.4 percent. This outcome was particularly surprising given that all of the other local bonds were approved, and a $1 billion state-wide bond to expand state prison capacity was overwhelmingly approved (58.2 to 41.8). The local campaign against the jail expansion proposition suggested that the intended location of the new jail would be bad for local economic development and that existing infrastructure could be more efficiently used with less reliance on incarceration. Some commentators explicitly placed the responsibility of the overcrowding problem on the courts in Harris County, citing an over-use of incarceration at the cost of taxpayer funds (Snyder 2007, Henson 2007a,b).

Most Harris County criminal courts exhibited a discernible change in their caseload outcomes immediately following the election. Diversion among first-time felony defendants increased by 18 percentage points from less than 50 percent of the caseload to roughly 65 percent (Figure 1). While we have not been able to verify this with the local courts, our interpretation is that the courts were responding to their critics given that both the district attorney and criminal court judges are publicly elected officials.[6]

The fact that the defeat of the jail expansion was unanticipated limits the likelihood of systematic sorting. Furthermore, disposition dates are typically scheduled well in advance constraining the ability to sort. As shown in Section 5, it does not appear that widespread sorting occurred around the election. Instead, it appears that some defendants were simply lucky to have been scheduled to be disposed after the election rather than before.

million in potential bonds. The projects included upgrading roads and parks, expanding capacity at the port, building a new forensic lab and constructing a new family law center.

[6]We have also considered an alternative theory for the 2007 change in conviction rates. On October 31, 2007, the Harris County District Attorney was served a subpoena associated with the *Ibarras v Harris County Texas* civil lawsuit regarding improper coaching of defense witness testimony. On Nov 5, 2007, an inventory of his official email was collected, some of which were improperly released to the public in December 2007 showing the DA was having a romantic affair with his executive secretary (Rogers et al. 2007). Additional emails were released on January 8, 2008, which showed the DA had exchanged dozens of pornographic, racist and political e-mails on his office computer (Rogers et al. 2008), which led to his resignation on February 15, 2008 (Lozano 2008). While the initiation of these events in the DA's office coincide with the 2007 cutoff we examine, it is unclear how these could lead to lasting changes in diversion practices in the courts extending out to 2009.

3 Sample Construction and Measurement of Outcomes

3.1 Administrative Data Sources

We rely on several county and statewide sources of administrative data in this study. From the criminal justice system, these include criminal court records from the Harris County District Clerk, jail booking and spell data from Harris County Sheriff's Department, state prison data from the Texas Department of Criminal Justice, and the Texas Department of Public Safety's Computerized Criminal History (CCH) database which tracks state-wide convictions in Texas.

The Harris County criminal court records contain charges (felony and misdemeanor) and court outcomes for all adults between 1980 and 2017 regardless of the final verdict.[7] We use this data to construct the core analysis sample, measure our source of identifying variation (date of charge or disposition), document first-stage diversion outcomes and their associated sanctions, and track future criminal activity. To assess the robustness of our recidivism measure, we also examine Harris County Jail bookings as a proxy for arrests and the statewide CCH conviction database to evaluate spatial spillovers. While each data source has potential limitations,[8] the findings remain remarkably consistent.

We are also able to observe time spent in the Harris County Jail and the Texas prison system. We cannot reliably link time served though to a specific charge or conviction. As a consequence, any changes in realized incarceration could be the result of initial sentencing disparities as well as future criminal behavior.

The two Harris County datasets are linked using a unique county identifier tied to an individual's fingerprint known as the "system person number" (SPN). We match this data to statewide data (CCH and the state prison records) using a defendant's full name and date of birth.[9]

To evaluate the impact of these different sanctions on labor market outcomes, we also match offenders to administrative earnings and employment data drawn from quarterly unemployment insurance wage records between 1994 and 2017 from the Texas Workforce

[7] Cases sealed to the public by order of the court, which account for less than half of one percentage point of the overall caseload, and criminal appeals were not included in the data.

[8] The Harris County Jail records can also contain multiple bookings associated with the same offense that may inflate the degree of criminal activity and previous audits have found the CCH data to have incomplete statewide coverage, particularly prior to the early 2000's, due to the voluntary nature of the reporting to the CCH.

[9] In 1994 (2007), 73.8% (81.8%) of the first-time felony offender sample ever match to a valid jail spell and 46.4% (30.0%) match to a valid prison spell.

Commission. Wage and employment records were matched to the criminal justice records using a Social Security Number (SSN).[10] To reduce the influence of extreme outliers we cap quarterly earning at $50,000.[11]

3.2 Sample Restrictions

We restrict our sample to first-time felony defendants charged two years before and after the Sept. 1, 1994 threshold and defendants disposed two years before and after the Nov. 7, 2007 threshold.[12] Through imposing the first-time offender restriction, we ensure that each defendant will only appear once in our estimation sample. In addition, individuals without a felony record are generally the target of diversion programs in the U.S. and so providing evidence for this group is particularly relevant and important from a policy perspective.

In 1994, we further limit the analysis to consider those charged with affected statutes as described in Section 2. In 2007, we focus on defendants at risk for incarceration in the county jail, who we believe would be most likely to be impacted by the 2007 ballot result.[13,14] We also apply a donut procedure in 2007 and drop all observations with disposition dates between October 30, 2007 and Nov. 9, 2007 to account for a transition period in diversion rates.[15] Online Table B.4 shows that eliminating this donut procedure reduces the size of the first stage relationship in 2007 by 33 percent.

[10] Approximately 77% (75%) of the 1994 (2007) sample has a recorded social security number. Individuals without a social security number on file were dropped from the labor market analysis. The unemployment insurance wage records do not contain name and date-of-birth preventing any form of probabilistic matching for those defendants without social security numbers.

[11] This affects less than 0.025% of person-quarters observed in the data.

[12] When computing our point estimates though, it should be noted that the effective sample window will be narrower due to the data-driven optimal bandwidth procedure described in Section 4.

[13] There is no significant change in the likelihood of being in the high or low-risk group across the 2007 threshold which should assuage concerns of endogenous sample selection.

[14] The motivation behind isolating the low-risk group from the high-risk group also comes from the fact that a $1 billion state-wide bond to expand state prison capacity, which was passed on the same 2007 ballot, increased both the sentenced length of incarceration as well as time served for those sentenced to the state prison system (99.8 percent of which fall into the high-risk group). As such, failing to distinguish between these two group creates a challenging pattern as one group experienced more lenient sanctions across the threshold while the other experienced more severe sanctions across the threshold (and had no change in diversion rates). To avoid this issue altogether, we simply focus our analysis on the low-risk group that should not have been impacted by the state-wide prison bond.

[15] An examination of the micro-data shows that different courts appear to have reacted to the pre-election debate and ballot outcome at different points in time which together create a transition period of approximately 2 weeks from one practice to another in the overall caseload. This may also relate to the events occurring in the District Attorney's office described in Footnote 6.

3.3 Imputation for Missing Follow-up Data

Although this paper leverages a wealth of administrative records, the timing of the available data makes it infeasible to observe outcomes over a full decade for each individual in the analysis. In the 1994 sample, we are missing initial labor market outcomes for those charged prior to Jan. 1, 1994; in the 2007 sample, we cannot observe end-of-decade criminal justice and labor market outcomes for those disposed after Oct. 1, 2007 and Jan. 1, 2008 respectively. While the missing data represents a minority fraction of the cumulative 10-year outcomes (see Table A.1), we do not want to ignore this issue as it could generate misleading trends in the graphical analysis as well as impact the bandwidth selection and bias-correction procedure in our RD analysis.

We impute the missing data using two types of panel models. In 1994, we backwards "forecast" missing quarters of earnings data using a Tobit model based on observed earnings starting in the first quarter of 1994.

$$Earnings_{i,t} = \begin{cases} Earnings^*_{i,t} & \text{if} \quad Earnings^*_{i,t} > 0 \\ 0 & \text{if} \quad Earnings^*_{i,t} \leq 0 \end{cases}$$

$$Earnings^*_{i,t} = \alpha^{94} + \sum_{q=1}^{6} \beta^{94}_q Earnings_{i,t+q} + \sum_{q=1}^{6} \gamma^{94}_q Work_{i,t+q} + \epsilon_{i,t}$$

In 2007, we utilize an equivalent procedure except forecasting earnings based on lagged values.

$$Earnings^*_{i,t} = \alpha^{07} + \sum_{q=1}^{6} \beta^{07}_q Earnings_{i,t-q} + \sum_{q=1}^{6} \gamma^{07}_q Work_{i,t-q} + \epsilon_{i,t}$$

The imputed values are iteratively computed by: (1) applying the fitted values for the α, β's and γ's to generate a linear prediction of $Earnings^*_{i,t}$, (2) adding a random shock $\hat{\epsilon}_{i,t} \sim N(0, \hat{\sigma}^2)$ based on the model's estimate of the variance of the error term in order to avoid overstating the precision of the estimates, and (3) censoring the results $\widehat{Earnings}^*_{i,t}$ at zero to generate $\widehat{Earnings}_{i,t}$.[16]

Table A.1 shows the share of observations for each sample, outcome variable, and follow-up year that include imputed values. To assess whether these imputations play an outsized role in our main results, we compute the contemporaneous impact of diversion on future convictions

[16] In the case of the imputed criminal justice outcomes in 2007, the model is slightly modified due to the relatively infrequent nature of criminal activity in the data. Instead we estimate an annual data model based on the follow latent variable: $Total\,Convictions^*_{i,t} = \alpha^{94} + \sum_{y=1}^{3} \beta^{94}_y Total\,Convictions_{i,t-y} + \sum_{y=1}^{3} \gamma^{94}_y Any\,Convictions_{i,t-y} + \epsilon_{i,t}$.

and employment separately by follow-up year in Figure A.1. We find that the level and trend of the contemporaneous impacts are consistent whether or not the follow-up data includes imputed values. We believe this demonstrates the robustness of our findings to the exclusion of the imputation procedure.

4 Research Design

We first estimate the impact of the 1994 and 2007 policy changes on diversion rates using a sharp RD design. We present both graphical evidence as well as statistical tests to confirm the reliability of our results. For our statistical tests, we follow the approach of Calonico et al. (2014, 2016a) to obtain bias-corrected point estimates using local linear functions, optimal bandwidths and valid confidence intervals. Formally, we estimate the discontinuity ($\hat{\tau}$) based on the following model:

$$\tau = \mu_+ - \mu_-,$$

where,

$$\mu_+ = \lim_{x \to 0^+} \mu(x), \quad \mu_- = \lim_{x \to 0^-} \mu(x), \quad \text{and} \quad \mu(x) \equiv E[Y_i | X_i = x].$$

The parameters μ_+ and μ_- represent the limit of the expectation of Y_i given X_i as it approaches the cutoff threshold from above and below respectively. As a result, τ should be thought to measure the magnitude of the jump in the outcome variable at the point of the discontinuity. In this notation, X_i is the running variable that has a cutoff threshold at $X_i = 0$ which generates a discontinuity in the outcome variable of interest (Y_i). Our running (or forcing) variable differs across the two quasi-experiments due to the nature of each change: the 1994 Penal Code Reform affected offenders based on the date the charge was filed; the 2007 change in court behavior was based on the date the case was first disposed by the court (i.e., given a verdict).

We parameterize $\mu(x)$ using a local linear function:

$$\hat{\tau}(h_n) = \hat{\mu}_{+,1}(h_n) - \hat{\mu}_{-,1}(h_n), \quad \text{where,}$$

$$\left(\hat{\mu}_{+,1}(h_n), \hat{\mu}^{(1)}_{+,1}(h_n)\right)' = \underset{b_0, b_1 \in \mathbb{R}}{\arg\min} \sum_{i=1}^{n} 1(X_i \geq 0)(Y_i - b_0 - X_i b_1)^2 K(X_i/h_n), \quad \text{and}$$

$$\left(\hat{\mu}_{-,1}(h_n), \hat{\mu}^{(1)}_{-,1}(h_n)\right)' = \underset{b_0, b_1 \in \mathbb{R}}{\arg\min} \sum_{i=1}^{n} 1(X_i < 0)(Y_i - b_0 - X_i b_1)^2 K(X_i/h_n).$$

K is the kernel function that determines the weighting scheme within a given bandwidth, while

h_n represents the size of the bandwidth itself. We opt for a data-driven bandwidth selector that selects the median bandwidth from three mean squared error-optimal methods for the RD treatment effect estimator,[17] and utilize the Uniform kernel function. Coefficients are estimated using a first-order local polynomial that has been bias-corrected using a second-order local polynomial with robust standard errors based on a heteroskedasticity-robust plug-in residuals variance estimator with HC_2 weights. Our primary specification adjusts for baseline covariates of age, gender, race/ethnicity, and prior number of misdemeanor convictions.[18] These parameterization decisions can be modified without impacting the findings of this study as documented in Online Appendix Tables B.2, B.3, and B.4.

After quantifying the "first-stage" relationship, we then turn to a fuzzy RD design to measure the causal effect of the diversion on court sanctions, future offending behavior, and labor market outcomes. In this latter approach, the measured change in outcomes is scaled by the observed first-stage relationship to quantify the impact of diversion. In doing this we assume that each threshold influences the outcomes measured only through it's impact on diversion (exclusion restriction) and that there are not individuals for whom the threshold is associated with an increase in diversion 1994 and decrease in 2007 (monotonicity assumption). We believe the specific context and institutional details discussed in Section 2 support these assumptions.

We also compute sample averages, which are presented with our estimates in brackets, in order to benchmark our results. For our sharp RD estimates, we present the overall caseload average in the bandwidth window prior to the cutoff. For our fuzzy RD estimates, we show an adapted control complier mean (CCM) (Kling et al. 2007, Heller et al. 2017) for the RD setting.[19] The CCM is the average outcome for the *complier* population (i.e. those caused to be diverted due to the discontinuity) in the absence of diversion (i.e. convicted). Please see Appendix A.1 for a discussion of our application of this method to the RD context.

[17] We use the option *msecomb2* within the STATA *rdrobust* command described by Calonico et al. (2016a) which uses the median bandwidth from the following methods: one common MSE-optimal bandwidth selector for the RD treatment effect estimator; two different bandwidth selectors (below and above); and one common MSE-optimal bandwidth selector for the sum of regression estimates.

[18] See Calonico et al. (2016b) for notation of this methodology including baseline covariates.

[19] To the best of our knowledge, this is the first paper to adapt the CCM estimator to the RD technique.

5 Empirical Results

5.1 Caseload Density and Baseline Characteristics

To attribute a causal interpretation to our RD estimates, we must assume that defendants are effectively randomly allocated before and after each threshold: individuals charged immediately before Sept. 1, 1994 should be observationally equivalent to those charged after and we should not see a discontinuity in the total number of cases; likewise, defendants disposed immediately on or after Nov. 7, 2007 should be observationally equivalent to those disposed before and we should not see a discontinuity in the total number of dispositions.

A sharp general deterrence response to the change in diversion rates would violate this assumption. Additional threats to our empirical strategy include changes in policing practices or sorting of offenders by prosecutors/judges across the threshold dates in order to guarantee they face one punishment regime versus the other. This is particularly concerning in the context of the 1994 reform since all relevant actors could fully anticipate the adoption of the new penal code. Each of these threats generate a testable prediction of discontinuous changes in either the size or composition of the criminal caseload across the discontinuity.

In spite of these potential threats to identification, we do not observe discontinuities in caseload densities or in baseline defendant demographic characteristics including gender, age, race, and ethnicity as well as misdemeanor criminal history. Balance across these factors is presented in Table 1, with supporting graphical evidence available for each estimate in Online Figure B.1. We do observe a marginally significant increase in the likelihood of being male of 4 percentage points after the 1994 cutoff, but no significant difference is observed in 2007. The third column of the table reports the combined estimate, $\frac{\tau^{07}-\tau^{94}}{2}$, which measures whether any systematic sorting towards a higher probability of diversion is detectable when averaging across the two experiments. Again, we do not see any evidence of endogenous sorting that contradicts our identification assumptions.

As a further test, we calculate a predicted recidivism risk score for each defendant using the baseline characteristics and prior number of misdemeanor convictions.[20] Since no information

[20] We calculate our measure of recidivism risk from the predicted dependent variables for each individual from the following OLS regression: $\text{Total Charges}_i^{10\text{ Years}} = \alpha + \mathbf{X}_i'\beta + \epsilon_i$, where $\mathbf{X}_i'$ is the set of the observable covariates (i.e., age, sex, race/ethnicity, and prior misdemeanor convictions — not the forcing variable) as well as the corresponding two-way interactions. The risk score is defined as $\hat{\alpha} + \mathbf{X}_i'\hat{\beta}$ and captures an offender's predicted rate of recidivism over ten years based on their observable characteristics. If police or prosecutors act in a discriminatory manner and monitor certain sub-populations at higher than average rates (e.g., African American men), then an alternative interpretation of this index would be having a higher or lower likelihood of involvement with the criminal justice system whether through differences in actual future behavior or

from the running variable or discontinuity is used in constructing this index, the assumptions of the RD research design would imply that no sharp changes in the predicted risk of recidivism should appear at the threshold, which is what we observe empirically in Table 1.

Continuity in the caseload density, demographic composition, and predicted recidivism risk strongly support our identification assumption of continuity in unobserved determinants of our outcomes across the threshold. We do, however, estimate a significant difference in missing a SSN in the 2007 sample. Upon further investigation, this difference appears to be driven by an increase in the proportion of case dismissals following the Nov. 2007 ballot, who have a higher likelihood of missing a SSN. While any imbalance in observable characteristics across the threshold is a concern, this specific imbalance will only impact our labor market analysis, which we believe will be downward biased against a finding of a positive impact of diversion since those with case dismissals tend to have lower-than-average recidivism risk which is associated with better labor market outcomes.

5.2 Court Verdicts and Sentencing Outcomes

The exact meaning of and counterfactual to diversion varies between the 1994 and 2007 experiments. Panel A of Table 2 reports the impact of diversion on case dispositions, while Panel B shows the effect on court-ordered sanctions. These point estimates are calculating using a fuzzy RD design and should be interpreted as the causal impact of diversion on disposition and sentencing outcomes.

In 1994, a diversion is entirely characterized as a deferred adjudication of guilt. We find a modest decrease in the likelihood of receiving an incarceration sentence or being issued a fine, but do not estimate any difference in the likelihood of probation.[21] As can be seen from the CCMs, marginal defendants not diverted received a felony conviction and a probation sentence, which is consistent with the probation-before-incarceration requirement created after the 1994 cutoff. While we observe a decrease in the likelihood of an incarceration sentence on file for those diverted, it is unlikely that these sentence differences reflect changes to sanctions in practice, which we confirm through our analysis of jail and prison records.

In 2007, what we term diversion is split roughly 60% and 40% between deferred adjudications of guilt and outright case dismissals respectively. While these are clearly different disposition outcomes, we have combined these rulings under the broader umbrella of diversion given that both decrease further involvement in the criminal justice system. By 2007, the

differences in future monitoring.

[21] Fuzzy RD estimates using the dollar amount of fines as an outcome variable imply that diversion in 1994 was associated with an insignificant decrease of $82 in fines on the margin.

probation-before-incarceration requirement had been eliminated, and the primary counterfactual to diversion is a felony conviction with an incarceration sentence. Here there is an unambiguous change in incarceration sentences as a consequence of diversion. Whether this translates into meaningful differences in time served depends on the length of sentence (which are relatively short by sample construction) and parole practices (which we cannot directly observe). From Table 2, it is clear that diversion in 2007 led to an increase in probation and/or a fine.[22] These estimated effects are less than a one-for-one exchange with incarceration due to the non-trivial share of defendants who received a dismissal-based diversion where no sentence would be applied.

Throughout our analysis, we test the average causal effect across 1994 and 2007 and equality between the two experimental estimates (the third and fourth columns respectively of tables reporting our RD results). These exercises are valuable from two perspectives. First, we can strengthen our statistical precision to detect impacts on high-variance behavioral outcomes through combining the estimates. Second, testing the difference between the coefficients quantifies the differences and similarities between the 1994 and 2007 estimates, which helps us distinguish between potential mechanisms driving the causal effects. Given the differences in the implementation of diversion across 1994 and 2007, it is unsurprising that our tests of equality are rejected across the board for case dispositions and sanctions in Table 2. When pooling the two natural experiments, deferred adjudications of guilt are the predominant case disposition associated with diversion, although 16 percent of marginal defendants still end up with a case dismissal. The sentencing impacts are generally more muted in the combined estimate, and the opposite-signed impacts on fines cancel each other out.

5.3 Reoffending and Labor Market Outcomes

Our main recidivism analysis, which evaluates the 10-year impacts of diversion on future convictions by extensive and intensive margins, by type of crime, and by offense level, are presented in Table 3. As with the disposition and sanction outcomes, the coefficients are estimated using a Fuzzy RD design and represent the causal impact of diversion.

In both 1994 and 2007, we find strong and consistent evidence that diversion leads to statistically significant and economically meaningful declines in recidivism (at both extensive and intensive margins). Diversion decreases reconviction rates by slightly more than 30 percentage points in both samples, which reflects close to a 50 percent reduction relative to the CCM. On the intensive margin, diversion reduces the total number of future offenses by

[22] Fuzzy RD estimates on the amount of fines show diversion in 2007 was associated with an insignificant increase of $130 in fines on the margin.

1.3 to 1.6 new convictions (56 to 77 percent). The pooled coefficients are similar in magnitude but more precise allowing us to rule out even small to modest impacts.[23] Unsurprisingly, we cannot reject that diversion has the same causal effect in the two separate analysis samples.

The change in the types of crimes prevented differ somewhat between 1994 and 2007. In 1994, it appears that the decrease in convictions is driven by lower rates of drug and property offenses. In 2007, drug offenses show a more modest effect while violent offenses exhibit a more substantial and statistically significant decline. In spite of these differences, we cannot statistically distinguish effects for specific types of crimes across the two natural experiments (as reported in the fourth column of Table 3). In fact, the pooled estimates show significant impacts on all types of crime reduction.

By level of the offense, it appears more felony-level crimes were prevented through diversion in 1994 (91 percent decline) whereas more misdemeanor-level crimes were prevented through diversion in 2007 (83 percent decline). But, here again, we estimate significant impacts on both margins in the pooled sample and statistical tests of the equality of the sample-specific coefficients fail to reject the null.

Our primary 10-year labor market results are presented in Table 4. This includes performance measured on three dimensions: employment rates (Panel A), total earnings (Panel B), and earnings stability (Panel C). Across both experiments and each group of outcomes, the evidence consistently shows that diversion has a positive causal impact on labor market performance.

We find that diversion increases quarterly employment rates by 20 (16) percentage points in 1994 (2007), indicating more than a 50 percent increase over the CCMs. Stated differently, diversion increases the average number of quarters with positive earnings from 13.6 quarters to 20.8 quarters in the complier population over ten years. These employment gains are not confined to occasional, very-low-income work; the second row of Panel A indicates significant improvements (13 percentage points) in quarterly employment rates with earnings exceeding the federal poverty level for a single adult.[24]

Table 4 also documents improved earnings overall as shown in Panel B. Averaging between the two experiments, we find that diversion increases log total earnings by 1.8 log points, and total earnings by $61,540. Taking into account the CCMs are around $95,000 over this period,

[23] In the pooled sample, we can reject that the impact of diversion is smaller than a 17 percentage point reduction in reconviction rates and a 0.79 reduction in total convictions based on a two-sided test with p-values less than 0.1.

[24] Although this result is encouraging, it is important to bear in mind that this represents a relatively modest target of just $3,035 per quarter in 2018.

the results represent quite substantial improvements for UI-covered income over baseline rates.

It is possible that these large differences in earnings are a result of decreases in job churn or other types of employment disruption. To explore this, we create two outcome measures of employment stability: first, we calculate the maximum continuous spell (in quarters) of employment with a specific employer over our 10-year follow-up period; and, second, we measure continuous spells of earnings, a version employment stability for the defendant that is not restricted to a single employer. Consistent with the prior labor market performance results, we find that diversion generates longer durations of continuous within-firm employment (3.7 quarters) and continuous periods of earnings (4.9 quarters). Increases in employer-specific experience or employment stability among those diverted likely contributes to the observed earnings growth over our follow-up period.

The 1994 experiment generally exhibits stronger statistical precision throughout Table 4. In particular, the impacts to employment rates in 2007 are marginally insignificant, which may raise questions about the robustness of these conclusions. Several pieces of evidence, however, stoke our confidence in these findings. First, across each of the labor market outcomes, the pooled estimates are highly precise with little doubt regarding the statistical significance. Because the pooled estimates integrate all of the identified exogenous variation into a single test, they are our best guess of the causal impact of diversion. Second, we fail to reject the null of treatment effect equality between 1994 and 2007 for all considered outcomes. Third, a number of alternative specification choices presented in our robustness exercises (see Tables B.2-B.4) do yield statistically significant improvements in employment rates as a result of diversion in 2007. Finally, the timing of the impacts presented in Figure 2, which will be discussed next, demonstrate significant effects on employment rates in 2007 between Years 1 and 8. This suggests the impact at Year 10 is somewhat less precise but unlikely to be the product of random noise.

Figure 2 plots the estimated cumulative impact of diversion on three outcomes (total future convictions, the quarterly employment rate, and total days incarcerated) over time. The first coefficient corresponds to the impact during the initial 365 days post-charge or disposition, and each sequential estimate adds another year of data to expand the total follow-up window up through 10 and 20 years for the 2007 and 1994 samples respectively.[25]

Both 1994 and 2007 plots show similar trajectories. We observe an immediate impact on criminal activity and employment that starts in the first year and grows year-by-year as the

[25] Online Figure B.6 reports a summary of the visual evidence of the reduced form discontinuities underpinning these results.

follow-up window is expanded. The first five years show particularly rapid improvements in crime reductions and employment gains, yet the benefits continue to accrue as far out as we are able to track outcomes. Although our main analysis focuses on 10-year outcomes, the persistent and growing impact up to 20 years out in the 1994 sample is quite remarkable. We interpret this pattern to indicate that diversion, at least at the critical juncture of someone's first felony charge, has the potential to fundamentally alter an individual's trajectory in life.

The final row of Figure 2 depicts the cumulative effect on time served in jail or prison. Given the previous discussion on sentencing impacts, it is useful to evaluate the effects on actual time served to assess whether changes to incarceration is a key mechanism for achieving diversion's impacts. The plots do not show any significant differences in cumulative time served until more than 10 years out in the 1994 sample; no significant effect is ever observed in the 2007 sample. Figure A.1, which shows the contemporaneous rather than cumulative effects on incarceration, documents an emergent trend starting in Year 6 for the 1994 sample. These impacts are most likely to be a byproduct of future criminal activity rather than an immediate consequence of the diversion itself given that this is well past the duration of most diversion agreements. The long-term divergence in time-served though raises the possibility that future sanctions for new convictions impact labor market and reoffending outcomes. As such, we cannot rule out incarceration as a potential indirect channel.

Our previous analyses consider criminal and labor market outcomes independently. The incidence of these impacts could fall on mutually exclusive subgroups, a common population or some combination thereof. To distinguish these possibilities, we create four mutually-exclusive outcomes for the 10-year follow-up period. We consider whether a defendant's quarterly employment rate is above or below 50% during the follow-up period, and whether they have zero of at least one new conviction during the follow-up period. The sample-specific and pooled results indicate substantial changes on the extreme outcomes (see Table 5). We estimate that diversion increases the fraction of individuals who jointly experience higher levels of employment and no future convictions by 25 percentage points, a gain of 232 percent over the CCM. At the same time, diversion reduces the fraction with low employment levels and at least one new conviction by 32 percentage points (66 percent). It hard to know whether these results reflect a major improvement for a narrow share of the marginal caseload or an incremental improvement for a broader group. Regardless of the interpretation, the impacts demonstrate substantial improvements in our sample for modest (if any) costs to the justice system.

5.4 Heterogeneity Analysis

Certain subgroups may particularly benefit from diversion. To explore this, we examine whether our estimated impacts differ over the spectrum of predicted recidivism risk.[26] We prefer this approach to the typical sub-group analysis because of the correlation between demographic traits in our sample. For example, black offenders are roughly two years younger than white offenders, so subgroup estimates by race may actually capture differences by age.[27]

We implement this analysis by separately estimating local polynomial RD specifications for each percentile in the risk score quantile function. Due to sample size constraints, we utilize a 40 percentile uniform bandwidth centered at the focal percentile when estimating these coefficients. Since the quantile function is not defined below zero or above one hundred, asymmetric bandwidths occur above the 80th and below the 20th percentiles.[28] Figure A.2 shows the prevalence of background characteristics over the smoothed risk score quantile function. Individuals at the highest predicted risk of recidivism are more likely to be young, African American, male, and have more prior misdemeanor convictions.

The first row of Figure 3 presents the first-stage relationship between diversion and the threshold across the 1994 and 2007 sample thresholds. The first stage magnitude remains fairly constant throughout the distribution. The second and third rows report the 10-year impacts on total convictions and average employment rates. The largest point estimates appear at the top end of the distribution in both samples, although improvements in in reoffending behavior also appear to show up among low-risk defendants as well. These results are striking in the fact that observable characteristics would predict that the high-risk defendants would have worse future outcomes, yet we find that a diversion effectively closes the employment gap between the 90th and median risk percentiles and halves the re-conviction gap between these groups. Two related mechanisms could generate these results. First, young, African American men may be over-targeted by law enforcement leading to a disproportionate treatment response in the marginal subpopulation. Second, the subpopulation may suffer more on average from a felony conviction record due to statistical discrimination or animus.

[26] See Section 5.1 for a description of the construction of this index. To account for the bias discussed in Abadie et al. (2016), the estimation of the risk score employs a leave-one-out or jackknife estimation procedure for the purpose of these exercises.

[27] We also report heterogeneous effects by sub-groups in Online Table B.1.

[28] This exercise requires the stronger assumption that defendants before and after the discontinuities exhibit similar risk score distributions. Figure B.8 shows a series of local polynomial RD estimates examining whether recidivism risk remains balanced within subsets of the risk score distribution. This provides a consolidated way to demonstrate balance throughout the distribution (as opposed being balanced on average in the sample overall as was previously documented in Table 1 and Figure B.1).

5.5 Robustness Checks and Placebo Exercises

The main reoffending analysis is based on Harris County conviction records. Table A.2 and Online Figures B.3 and B.4 demonstrate robustness of our results to alternative measures of recidivism: county jail bookings, county charges and statewide convictions. There does not appear to be any significant difference in the point estimates depending on the measure of recidivism. Given differences in the CCM by outcome, with bookings having the highest prevalence and convictions the lowest, the percent change does increase the closer one gets to conviction, but this also is not a statistically significant difference.

To address concerns regarding differential mobility out of Harris County, we compare Harris County and non-Harris County reoffending outcomes in Texas using the state-wide Computerized Criminal History (CCH) Database (Table A.3). If diversion displaced criminal activity to other counties in Texas, we should expect to see a negative impact in Harris County and a positive impact elsewhere in the state. In contrast, the results show a decline in reoffending both within and outside Harris County. Although the Harris County-specific convictions make up the majority of the observed impact in the state-wide CCH data, the effects elsewhere in the state suggest that our county-specific measures, if anything, underestimate the total gains from diversion.[29]

We conduct several additional robustness checks to confirm the reliability of our results. Table A.4 presents estimated effects of a placebo experiment where we shift the cutoff dates to one year earlier (Sept. 1, 1993 and Nov. 7, 2006) or one year later (Sept. 1, 1996 and Nov. 7, 2008) than our actual threshold dates. The goal in these exercises is to investigate whether our research design is identifying latent seasonal breaks caused by other factors such as the start of the school year or holiday season. Overall, we do not find any meaningful discontinuities in initial court outcomes, future reoffending, or future labor market outcomes in these placebo exercises.

We also provide a number of other robustness checks including specifications using several alternative bandwidth selection methods (Online Table B.2) and alternative variance estimation strategies (Online Table B.3). Online Table B.4 reports results for several other modifications including models without the donut restriction, without including covariates, with alternative kernels, and without using the robust standard errors and bias correction suggested by Calonico et al. (2014). We also provide estimates from models based on outcomes

[29] While these state-wide results partially alleviate mobility concerns, there could be differences in defendants being forced to leave Texas. If Hispanic immigrants are more or less likely to be deported, this could bias our estimated effects. However, we expect this bias to go in the opposite direction of our estimated effects as those receiving a felony conviction compared with a diversion would be more likely to be deported.

aggregated to the week level. Among these tables, there is no evidence to suggest our findings are driven by specific choices we made in implementing the RD methodology.

6 Discussion

An important question remains as to how this low-cost intervention could generate such large and long-term improvements. The magnitude of our estimates are particularly surprising given that prior evaluations of reentry services (as recently reviewed by Doleac (2018)) or summer jobs (Heller 2014, Gelber et al. 2015) have failed to yield impacts with similar magnitudes or degrees of persistence despite requiring substantially greater resources.

Our analysis of sentencing outcomes raises the possibility that fewer incarceration sentences heavily influence our estimated impacts. Impacts on actual time served over our follow-up period, however, suggest that this is unlikely. We also find that diversion significantly impacted financial penalties, yet these impacts go in opposite directions in 1994 and 2007, which is inconsistent with the pattern of results we observe for labor market and reoffending outcomes. Our results do not suggest that incarceration or fines play a key role.

Instead, we believe two direct and one indirect channels are likely the key mechanisms at work. The first direct channel is the avoidance of a felony conviction. The defendants in our analysis sample have no prior felony charges, and so diversion represents an opportunity to avoid the lifelong stigma of a felony record. Increasing evidence suggests that a felony conviction carries sharp penalties for employment opportunities (Raphael 2014, Finlay 2009, Pager et al. 2009, Pager 2003). A stigma effect may also exist within the criminal justice system if police, prosecutors, or judges react to prior felony convictions. This may operate mechanically due to penal code escalations or risk classification algorithms that depend on criminal conviction history.[30]

A natural way to evaluate this channel is to compare our estimated impacts with the corresponding coefficients for defendants with a felony record. Diversion, however, is rarely offered to those with a prior felony, which limits our ability to implement this strategy. Table A.5 compares our main RD estimates (both sharp and fuzzy) with the corresponding coefficients for defendants with 1 to 3 prior felony convictions. The exercise suggests that defendants without a felony record disproportionately benefit from diversion, which would be

[30]For instance, certain types of prior convictions can trigger aggravated (elevated) charges upon reoffense which results in more severe and potentially more certain future convictions. Additionally, defendants under varying forms of supervision may face differing probabilities of arrest conditional on criminal activity. We are not aware of any empirical evidence that confirms this to be the case however.

consistent with the proposed channel, but the exercise is limited by a few key factors. First, in 1994, there is no discernible first-stage relationship. Consequently, there is no way to compute treatment effect estimates for this cohort. In 2007, we do observe a statistically significant yet modest first stage relationship in the repeat offender caseload. The point estimates in fact suggest that diversion does not generate the same gains for this population, however, large standard errors, which are partially a consequence of the modest first stage, limit our ability to make this claim definitively.

The second direct channel is a form of deterrence that we term *re-sentencing deterrence*. If a defendant violates their diversion agreement, which on average last 3.5 years in the sample, they can be convicted and sentenced at that time for their original offense.[31] This threat would discourage reoffending during the agreement period, particularly for minor, non-felony offenses.[32] We do observe substantial declines in misdemeanor-level offenses as well as sizable impacts during the immediate follow-up period, a pattern that is consistent with re-sentencing deterrence as a contributing mechanism. Our impacts, however, continue to grow long after the diversion agreements expire. Moreover, we also find substantial reductions in felony convictions despite the weaker deterrence effect relative to less-serious offenses. Together these results lead us to conclude that re-sentencing deterrence alone cannot fully account for the results.

Finally, we believe an indirect mechanism, which we term an *amplification effect*, also contributes to the estimated long-run impacts. Any initial changes to criminal or labor market activity may dynamically influence future outcomes. For instance, if a felony conviction causes additional criminal activity and this results in additional sanctions against the individual, the initial treatment differences between diversion and conviction dispositions may be reinforced or amplified. Stated differently, effective dosage may grow over time as a consequence of the behavioral outcomes.

Figure A.3 shows direct evidence that amplification is plausible. This figure tracks the cumulative impact of diversion on new sanctions for future criminal activity, and shows that diversion decreases the total number of future incarceration and probation sentences. The effects are larger for incarceration but both impacts grow over the follow-up period. Our findings also support the hypothesis that amplification affects behavioral outcomes. First,

[31] This only applies to individuals who receive a deferred adjudication of guilt disposition.

[32] A new minor misdemeanor conviction (e.g., shoplifting) would result in a comparatively outsized sanction (felony conviction record and potentially time in prison) due to the sentence imposed by the violation of the deferred adjudication agreement, whereas a new serious felony conviction (e.g., aggravated assault) would trigger a felony conviction and harsh sanctioning regardless of whether a deferred adjudication was active at the time.

the cumulative behavioral impacts grow over time (as documented in Figure 2) suggesting a compounding effect of new sanctions over time. Second, the intensive-margin impacts on reoffending exceed the extensive-margin effects, suggesting that criminal risk builds on itself.

The precise nature of an amplification effect is hard to pin down. Evolving differences in incarceration raise the possibility of incapacitation and post-release effects. Changing labor market opportunities alter the opportunity cost of criminal activity. A host of other interventions and incentives may come into play. Without further structure imposed on the analysis, we are not able to disentangle these channels. Regardless, it is important to remember that diversion remains the catalyzing force behind the chain of events and the resulting divergent trajectories.

7 Conclusion

This paper studies two sharp discontinuities in criminal court diversion — a cost-saving strategy that shields defendants from further interaction with the criminal justice system — among first-time felony defendants in Harris County, Texas. While these two changes occurred 13 years apart and originate from different contexts, we find large and consistent impacts from both experiments. Recidivism and employment rates improve by around 50 percent. To the best of our knowledge, no prior research has found similarly large and long-lasting effects for such a low-cost intervention.

Heterogeneity analysis demonstrates a striking pattern: those at the highest risk of recidivism gain the most from diversion. These individuals are typically young, black men with one or more misdemeanor convictions, a group often discussed as over-policed in the United States. Our results indicate that intervening for this hard-to-serve population at a critical moment (i.e., when they are being charged with their first felony offense) could significantly improve their life course. More broadly, these results suggest the criminal justice system likely plays a crucial role in the propagation of racial inequality in the U.S.

We consider the evidence presented to be highly rigorous. The research design addresses selection bias without relying on controlling for observables or propensity score matching. The results are shown to be robust to a variety of specification choices as well as several placebo exercises. The findings are replicated in two independent samples, which together exhibit both increasing and decreasing rates of diversion. As such, it is unlikely that our analysis is biased by some unobserved shocks contemporaneous with the discontinuous changes in diversion.

In 2010, annual justice expenditures eclipsed $250 billion in the U.S. (Kyckelhahn 2010). Given the impact on federal and state budgets, substantial efforts have been made to reform criminal justice policy. The general trend has been towards more leniency, especially for first-time and low-risk defendants, with diversion emerging as a popular tool. Our results suggest that these changes may lead to lower rates of reoffending and higher rates of rehabilitation in the coming years.

Recent instructions by the U.S. Attorney General, however, have sought to reverse course in the federal justice system (Sessions 2017). This could reflect an alternative, risk-averse perspective that seeks to minimize the possibility of high-cost rare events (e.g. inadvertently diverting a to-be murderer) even at the cost of average gains. Our study is not well-equipped to quantify the impacts on these rare events. We do find a decline in violent crimes overall, however, which should be attractive to those coming from this perspective.

While much has been recently written on ineffective criminal justice policy in the U.S., this paper provides rigorous evidence on a successful intervention that both improves defendant outcomes and saves public resources. What we find most attractive about diversion is that it can be feasibly implemented without significant investments or changes to current infrastructure, making it a practical solution for criminal justice reform. While it is not clear whether those with a prior felony record would similarly benefit from diversion, the results are unambiguous for those facing their first felony charge. We believe diversion should be viewed as an attractive option for jurisdictions seeking to reduce the fiscal cost and community impact of their criminal justice system.

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Figures

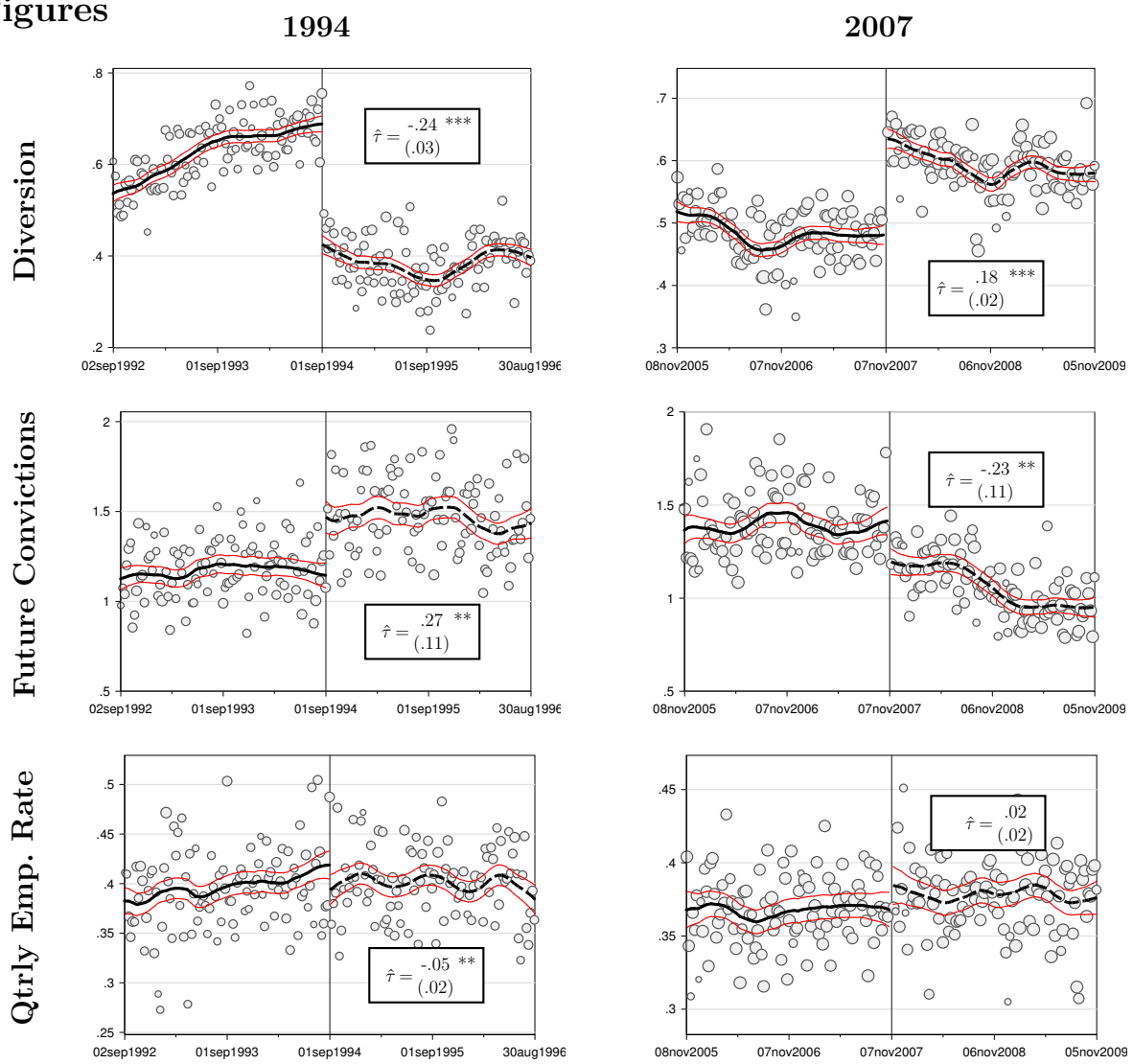


Figure 1: Graphical summary of the reduced form evidence in both the 1994 and 2007 samples

This figure presents reduced form graphical evidence of the changes in diversion, reoffending, and employment associated with the 1994 (left column) and 2007 (right column) natural experiments. The first row depicts the relationship between the running variable and diversion, defined as a court verdict of a deferred adjudication or guilt or a case dismissal. The next two rows plot our key outcomes measured over a ten-year follow-up period: the total number of future Harris County convictions; and, the average quarterly employment rate.

General RD Figure Notes: In general, we present reduced form (sharp RD) results in panels of figures with the left and right columns representing the 1994 and 2007 experiments, respectively. The threshold dates for each experiment are 9/1/1994 (based on the date the charge is filed) and 11/7/2007 (based on the date the case is disposed) and we include two years of data on each side of the threshold date. Scatter points represent week-level bin averages and are weighted by the total number of individual cases. We overlay local polynomial lines and their associated 90 percent confidence bands weighted using an epanechnikov kernal and 90-day bandwidth. We also report reduced form local-polynomial regression-discontinuity point estimates ($\hat{\tau}$) and standard errors in each plot. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

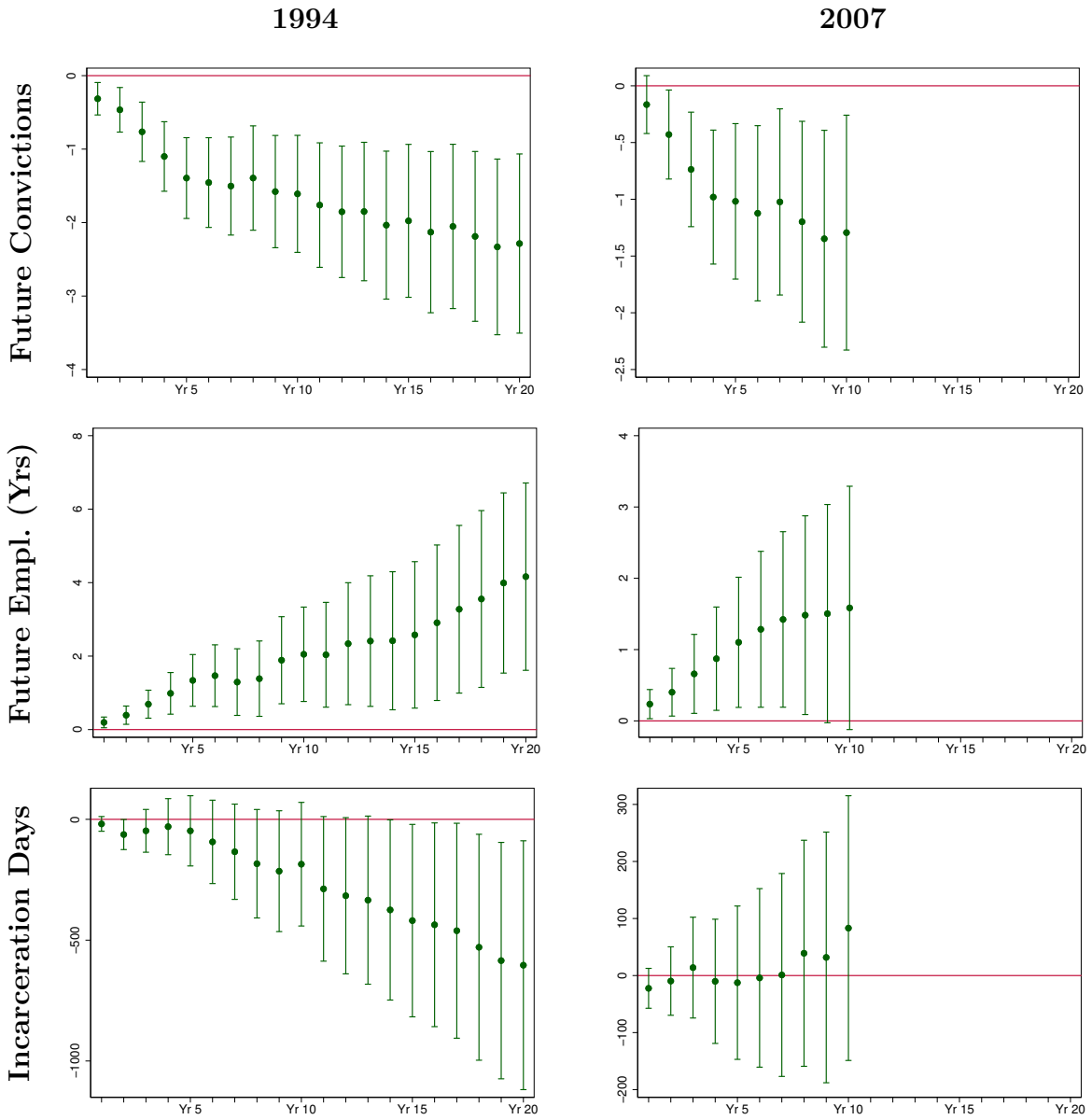


Figure 2: Timeline Impacts

This figure displays fuzzy RD estimates and associated 90% confidence bands for reoffending, employment, and incarceration outcomes that measure cumulative impacts of diversion after each year up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The first two rows depict the estimated impact of diversion on our key outcomes up through each year indicated on the horizontal axis: the total number of future Harris County convictions; and, the average quarterly employment rate. The third row reports the estimated impact of diversion on incarceration days. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2. Contemporaneous (year-by-year) estimates are presented in Figure A.1, and visual, reduced-form evidence is available in Online Appendix Figure B.6.

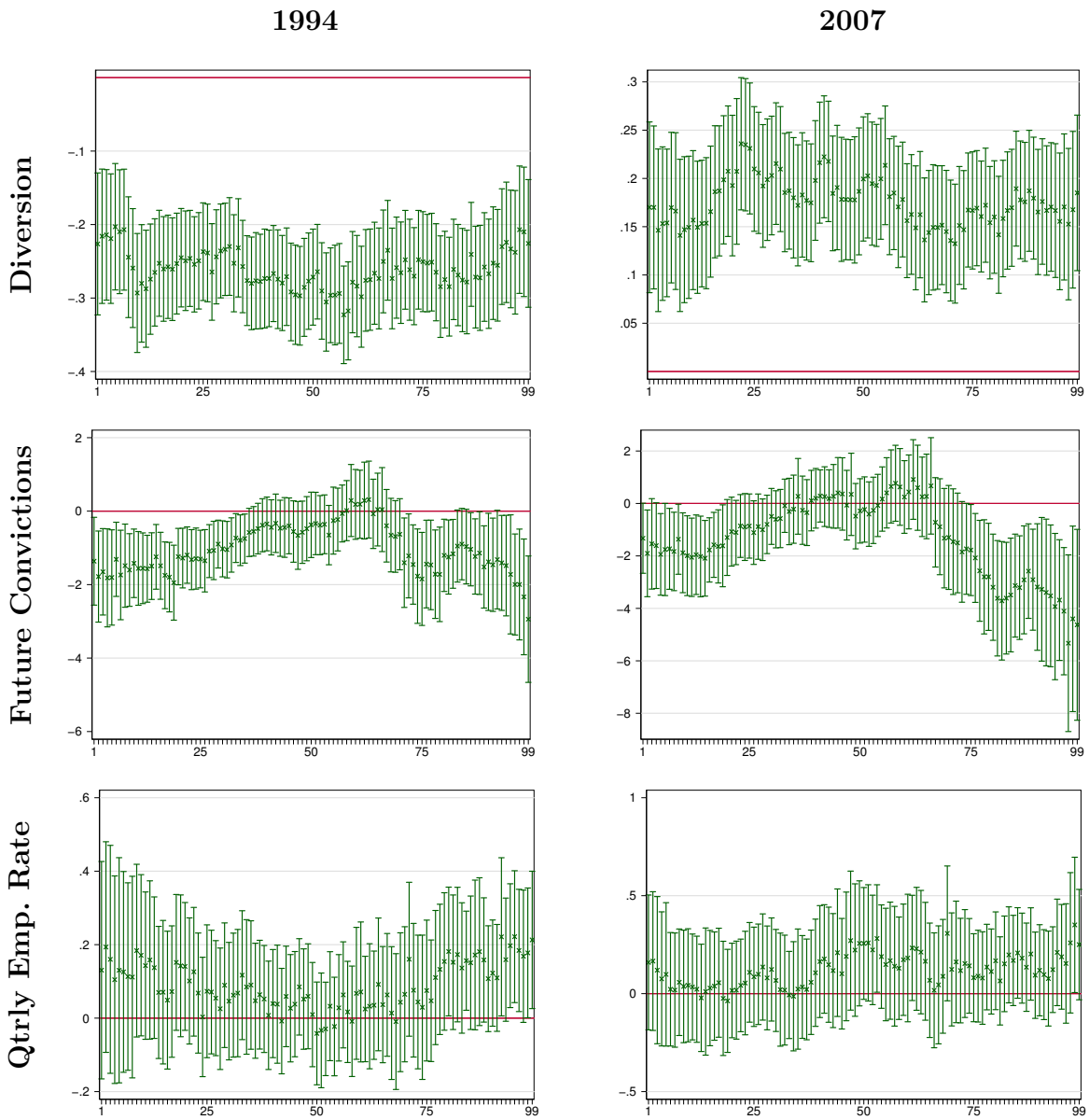


Figure 3: First-Stage and fuzzy RD Impacts over the Recidivism Risk Profile

This figure displays RD estimates and associated 90% confidence bands for diversion, reoffending, and employment outcomes calculated over the quantile function of the predicted recidivism risk score described in Section 5.4 for the 1994 experiment (left column) and 2007 experiment (right column). The first row presents first-stage estimates of the impact of the threshold date on the probability of diversion, defined as a court verdict of a deferred adjudication or guilt or a case dismissal. The next two rows plot fuzzy RD estimated impacts of diversion on our key outcomes measured over a ten-year follow-up period: the total number of future Harris County convictions; and, the average quarterly employment rate. Each coefficient reflects a distinct local polynomial RD estimate from regression centered at the focal percentile using a uniform kernel with a 40 percentile bandwidth. Estimates below the 20th and above the 80th percentiles will reflect narrower, asymmetric bandwidths. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Tables

Table 1: Balance Tests

	τ^{94}	τ^{07}	$\frac{\tau^{07} - \tau^{94}}{2}$
Caseload Size	-3.730	-2.240	0.750
	(2.520)	(3.570)	(2.190)
	[28.6]	[50.4]	
Total Prior Misd. Conv.	-0.006	0.033	0.020
	(0.058)	(0.052)	(0.039)
	[0.55]	[0.70]	
Age at Charge	-0.460	0.500	0.480
	(0.490)	(0.550)	(0.370)
	[28.8]	[29.4]	
Sex = Male	0.044*	0.023	-0.011
	(0.027)	(0.020)	(0.017)
	[0.68]	[0.73]	
Race/Ethn. = Black, not Hisp.	-0.036	-0.027	0.004
	(0.027)	(0.022)	(0.017)
	[0.46]	[0.38]	
Race/Ethn. = Hispanic	-0.020	0.025	0.023
	(0.023)	(0.021)	(0.016)
	[0.21]	[0.31]	
Crime Type = Property	0.003	0.010	0.003
	(0.027)	(0.022)	(0.017)
	[0.54]	[0.28]	
Crime Type = Drug	0.000	-0.015	-0.008
	(0.028)	(0.026)	(0.019)
	[0.46]	[0.45]	
Crime Type = Violent	–	-0.006	–
		(0.014)	
		[0.10]	
Recidivism Risk Score	0.069	-0.029	-0.049
	(0.043)	(0.042)	(0.030)
	[1.27]	[1.29]	
Social Sec. Number Unrecorded	0.001	0.047**	0.023
	(0.022)	(0.022)	(0.015)
	[0.22]	[0.22]	
Observations	31,254	52,701	.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents sharp RD estimates for pre-determined characteristics as recorded in the criminal court records from the Harris County District Court. The calculation of the recidivism risk score is described in Section 5. The first two columns report results from the 1994 and 2007 experiments, respectively. These estimates correspond to plots in Online Appendix Figure B.1. The third column calculates the average imbalance across the two experiments between the low-diversion and high-diversion period to assess whether there is any combined evidence of differential sorting. This calculation subtracts the 1994 point estimate since diversion was reduced in this experiment.

General RD Table Notes: Unless otherwise noted, all tables present local-polynomial regression discontinuity point estimates, standard errors in parentheses, and control-group means in square brackets. For all sharp RD estimates, control group means represent the average for defendants just to the left of the threshold dates within the data-driven bandwidth. For all fuzzy RD estimates, we report *complier control means* (CCM) as described in Section 4. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Table 2: Impact of Diversion on Disposition and Sentencing Outcomes

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
Panel A: Case Disposition				
Def. Adjudication of Guilt	1.080***	0.590***	0.830***	0.490***
	(0.098)	(0.100)	(0.071)	(0.140)
	[0.00]	[0.00]		
Case Dismissal	-0.100	0.410***	0.160**	-0.520***
	(0.100)	(0.099)	(0.071)	(0.140)
	[0.00]	[0.00]		
Panel B: Sentencing				
Incarceration Sentence	-0.190**	-0.970***	-0.580***	0.790***
	(0.086)	(0.056)	(0.051)	(0.100)
	[0.16]	[1.00]		
Probation Sentence	-0.017	0.670***	0.330***	-0.690***
	(0.110)	(0.110)	(0.079)	(0.160)
	[1.00]	[0.00]		
Fined	-0.300**	0.460***	0.081	-0.760***
	(0.120)	(0.130)	(0.089)	(0.180)
	[0.96]	[0.00]		
Drug Treatment	0.032	0.083	0.058	-0.051
	(0.045)	(0.068)	(0.041)	(0.081)
	[0.030]	[0.00]		
Observations	31,131	52,792		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the effect of diversion on case dispositions and sentencing for the focal felony charge. The first two columns report results from the 1994 and 2007 experiments respectively. The third column calculates the average impact across the two experiments and the fourth column tests whether we can statistically distinguish our fuzzy RD estimates for the 1994 experiment from the corresponding estimates for the 2007 experiment.

Table 3: Impact of Diversion on Reoffending Behavior over 10 years

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
Panel A: Overall				
Any Convictions	-0.310***	-0.320**	-0.320***	0.013
	(0.120)	(0.140)	(0.091)	(0.180)
	[0.70]	[0.64]		
Total Convictions	-1.610***	-1.290**	-1.450***	-0.320
	(0.480)	(0.630)	(0.400)	(0.790)
	[2.09]	[2.31]		
Panel B: Crime Type				
Drug Convictions	-0.710***	-0.190	-0.450**	-0.520
	(0.260)	(0.240)	(0.180)	(0.350)
	[0.80]	[0.63]		
Property Convictions	-0.500**	-0.470	-0.480**	-0.039
	(0.220)	(0.340)	(0.200)	(0.410)
	[0.56]	[0.70]		
Violent Convictions	-0.110	-0.280*	-0.190**	0.170
	(0.099)	(0.150)	(0.089)	(0.180)
	[0.24]	[0.30]		
Panel C: Offense Level				
Misdemeanor Convictions	-0.640**	-1.090**	-0.870***	0.460
	(0.290)	(0.450)	(0.270)	(0.540)
	[0.94]	[1.32]		
Felony Convictions	-1.070***	-0.510	-0.790***	-0.560
	(0.300)	(0.370)	(0.240)	(0.470)
	[1.18]	[1.04]		
Observations	31,131	52,792		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the impact of diversion on reoffending outcomes measured using a 10-year follow-up period. The first panel reports results with the total number of convictions in the ten-year follow-up period at the extensive (*Any Conviction*) and intensive (*Total Convictions*) margins. Panel B splits total convictions by type: drug, property, and violent. Panel C reports results separating convictions by severity. *General RD Table Notes* from Table 1 apply.

Table 4: Impact of Diversion on Labor Market Outcomes over 10 years

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
Panel A: Employment				
Qtrly Employment Rate	0.200***	0.160	0.180***	0.047
	(0.078)	(0.100)	(0.065)	(0.130)
	[0.37]	[0.31]		
Earn $\geq$ 100% Fed. Pov. Level	0.140**	0.120	0.130**	0.020
	(0.069)	(0.095)	(0.059)	(0.120)
	[0.23]	[0.21]		
Panel B: Earnings				
Log Avg. Qrtly Earnings	1.450	2.210*	1.830**	-0.760
	(1.020)	(1.340)	(0.840)	(1.680)
	[8.73]	[7.87]		
Avg. Qrtly Earnings	82,262**	40,818	61,540*	41,444
	(38,649)	(53,793)	(33,119)	(66,238)
	[91,340]	[101,363]		
Panel C: Tenure				
Max Employment Spell (Quarters)	1.510	5.810**	3.660**	-4.310
	(1.570)	(2.760)	(1.590)	(3.180)
	[6.25]	[5.88]		
Max Cont. Earning Spell (Quarters)	3.440	6.400*	4.920**	-2.960
	(2.290)	(3.580)	(2.120)	(4.250)
	[9.85]	[8.18]		
Observations	24,042	39,674		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the impact of diversion on labor market outcomes measured using a ten-year follow-up period. The labor market outcomes vary across each panel. Panel A presents estimated effects on the quarterly employment rate in the first row and quarterly employment at or above the federal poverty level in the second row. All wage outcomes are adjusted to 2018 US dollars using the Houston MSA CPI index and the 2018 Federal Poverty Level for a single adult is ($12,140). Panel B presents results on a average quarterly earnings dependent variable in log form (first row) and the raw level (second row). Finally, Panel C presents results for two dependent variables measuring employment stability by calculating the longest spell (measured in total quarters) of uninterrupted employment at a single employer (first row) or consecutive earnings (second row) during the follow-up period. *General RD Table Notes* from Table 1 apply.

Table 5: Impact of Diversion on Intersection Outcomes over 10 years

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
Qtrly Employment Rate $\geq$ 50%; No Convictions	0.290***	0.200*	0.250***	0.086
	(0.100)	(0.120)	(0.080)	(0.160)
	[0.14]	[0.084]		
Qtrly Employment Rate $\geq$ 50%; 1+ Convictions	-0.038	0.027	-0.006	-0.065
	(0.088)	(0.110)	(0.071)	(0.140)
	[0.22]	[0.15]		
Qtrly Employment Rate $<$ 50%; No Convictions	0.098	0.084	0.091	0.014
	(0.081)	(0.130)	(0.077)	(0.150)
	[0.14]	[0.29]		
Qtrly Employment Rate $<$ 50%; 1+ Convictions	-0.220**	-0.410**	-0.320***	0.200
	(0.093)	(0.160)	(0.094)	(0.190)
	[0.50]	[0.47]		
Observations	24,042	39,674		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the impact of diversion on four mutually-exclusive categories considering labor market outcomes and criminal offending outcomes jointly over the ten-year follow-up period. The first row estimates the probability of more-than 50% employment *AND* no future convictions over the ten-year follow-up period while the fourth row reports estimates of the impact of diversion on a less-than 50% employment rate *AND* at least one future conviction. The other two rows report effects on different combinations of these employment and reoffending measurements. General RD Table Notes from Table 1 apply.

A Print Appendix

A.1 Control Complier Means in the RD Design

For simplicity, let us assume that approaching the discontinuity from below represents the low diversion regime (i.e. control) and approaching the discontinuity from above represents the high diversion regime (i.e. treatment). Let C correspond to whether an observation is a complier or not. The CCM can thus be defined as:

$$CCM \equiv \lim_{x \to 0^-} \mu(x|C = 1)$$

The challenge in calculating this estimator is that complier status is not directly observable. What is observable, however, is the average outcome among the non-diverted population (i.e. $D = 0$) approaching the discontinuity from below. This will be a weighted average of the outcomes among compliers and *never-takers* (i.e. those never diverted regardless of the discontinuity), which we define as $N = 1$.

$$\lim_{x \to 0^-} \mu(x|D = 0) = \lim_{x \to 0^-} \mu(x|C = 1)\,(1 - \omega) + \lim_{x \to 0^-} \mu(x|N = 1)\,(\omega)$$

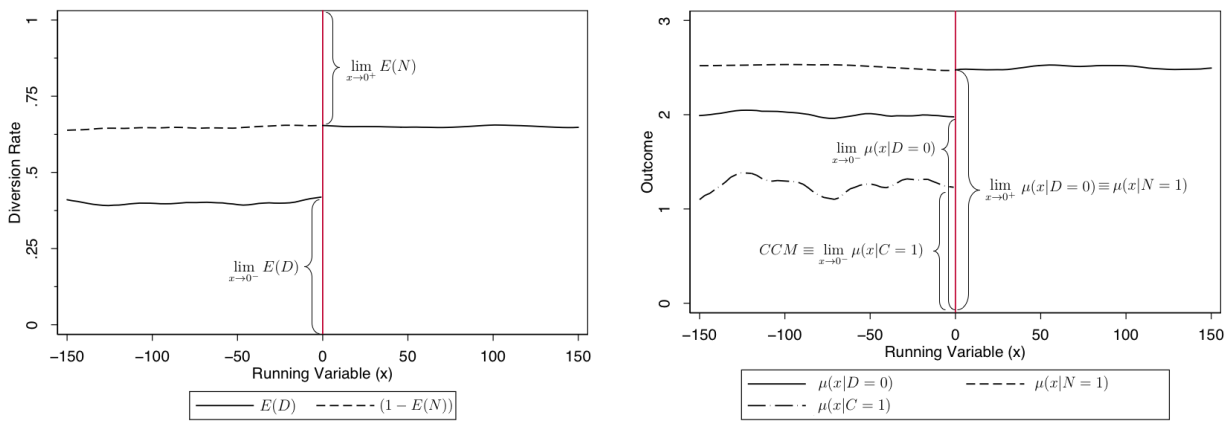
The weight (ω) indicates the never-taker share of the non-diverted population: $\frac{\lim_{x \to 0^-} E(N)}{1 - \lim_{x \to 0^-} E(D)}$.

While we can calculate the proportion ($1 - E(D)$) and outcome averages ($\mu(x|D = 0)$) for the non-diverted population as we approach the threshold from below, we cannot disentangle the contribution of never takers from that of the compliers. However, we can instead use information from those who are not diverted above the threshold since, by the monotonicity assumption of our RD design, this is the population of never-takers. This simply involves replacing $\lim_{x \to 0^-} E(N)$ and $\lim_{x \to 0^-} \mu(x|N = 1)$ with $\lim_{x \to 0^+} E(N)$ and $\lim_{x \to 0^+} \mu(x|N = 1)$ respectively. For the sake of simplicity and transparency, we approximate the local average approaching the cutoff in the equations discussed using a fixed bandwidth of 2.5 months and a uniform kernel.

Our illustration on the next page depicts the CCM procedure applied to a RD setting as described in Section 4. It utilizes artificial data for clarity. Solid black lines represent information that is directly observable to the econometrician. Dashed and dotted lines represent statistics that are not observable, but can be inferred based on the assumptions of the research design. Recall,

$$CCM \equiv \lim_{x \to 0^-} \mu(x|C = 1) = \frac{\lim_{x \to 0^-} \mu(x|D = 0) - \lim_{x \to 0^-} \mu(x|N = 1) \left(\frac{\lim_{x \to 0^-} E(N)}{1 - \lim_{x \to 0^-} E(D)} \right)}{\left(1 - \frac{\lim_{x \to 0^-} E(N)}{1 - \lim_{x \to 0^-} E(D)} \right)}.$$

Two of the right-hand side terms can be directly estimated without assumptions: $\lim_{x \to 0^-} \mu(x|D = 0)$ and $\lim_{x \to 0^-} E(D)$. The first term is the average outcome for those not diverted approaching the discontinuity from below. The second is the rate of diversion approaching



(a) Diversion rates in the overall caseload (b) Average outcomes in the non-diverted populations

An illustration of the control complier mean procedure

the discontinuity from below. Both can be observed because these are caseload-wide statistics and do not rely on unobservable statuses like being a *complier* or *never-taker*.

The remaining terms, $\lim_{x\to 0^-} \mu(x|N=1)$ and $\lim_{x\to 0^-} E(N)$, cannot be directly observed in the data. We can, however, instead approximate their values at cutoff using observations approaching the discontinuity from above: $\lim_{x\to 0^+} \mu(x|N=1)$ and $\lim_{x\to 0^+} E(N)$ respectively. This is feasible because only never-takers will not be diverted after the threshold based on the monotonicity assumption.

The figure highlights the pieces of information that are used to execute the CCM calculation. Together, these present our best estimate of the potential outcome that compliers would have experience in the absence of diversion.

A.2 Appendix Figures

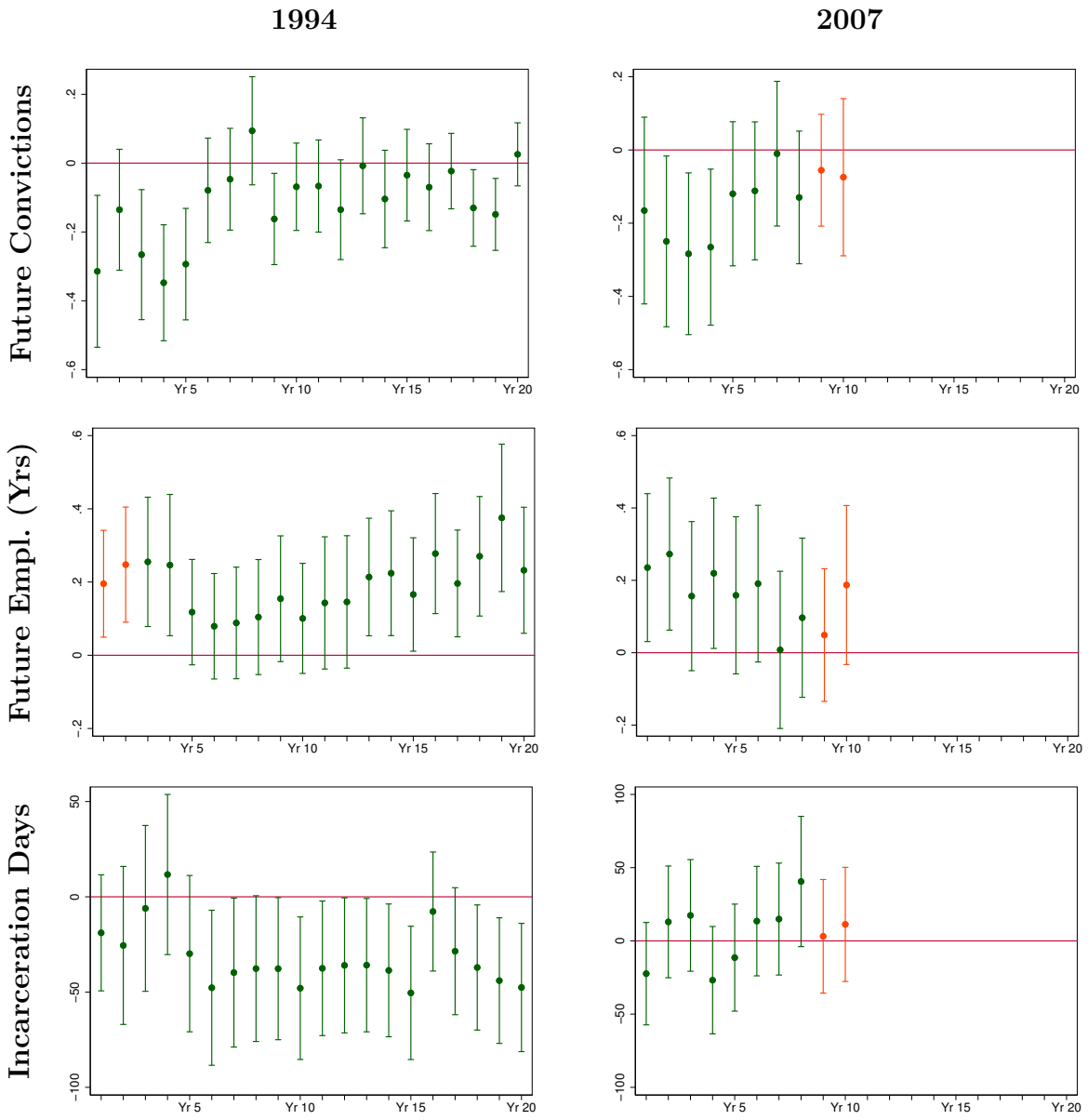


Figure A.1: Contemporaneous impacts by year of follow-up

This figure displays fuzzy RD estimates and associated 90% confidence bands for reoffending, employment, and incarceration outcomes that measure year-by-year (contemporaneous) impacts of diversion for each year period up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The first two rows depict the estimated impact of diversion on our key outcomes up through each year indicated on the horizontal axis: the total number of future Harris County convictions; and, the average quarterly employment rate. The third row reports the estimated impact of diversion on incarceration days. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2. Cumulative estimates are presented in Figure 2.

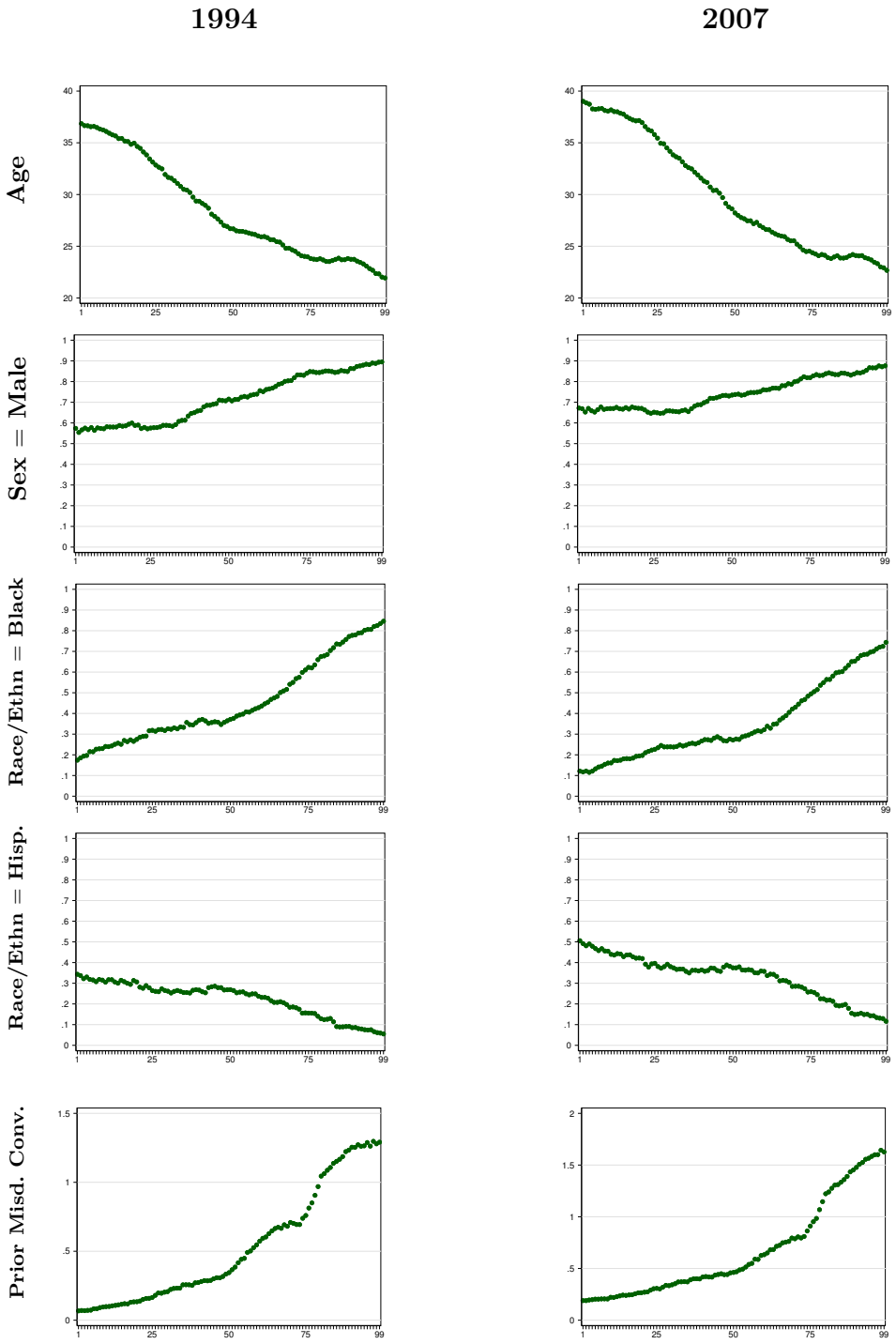


Figure A.2: Smoothed Defendant Characteristics over the Recidivism Risk Profile

This figure plots mean defendant demographic and criminal history characteristics over the quantile function of the predicted recidivism risk score described in Section 5.4. Estimated impacts of diversion across this distribution are presented in Figure 3.

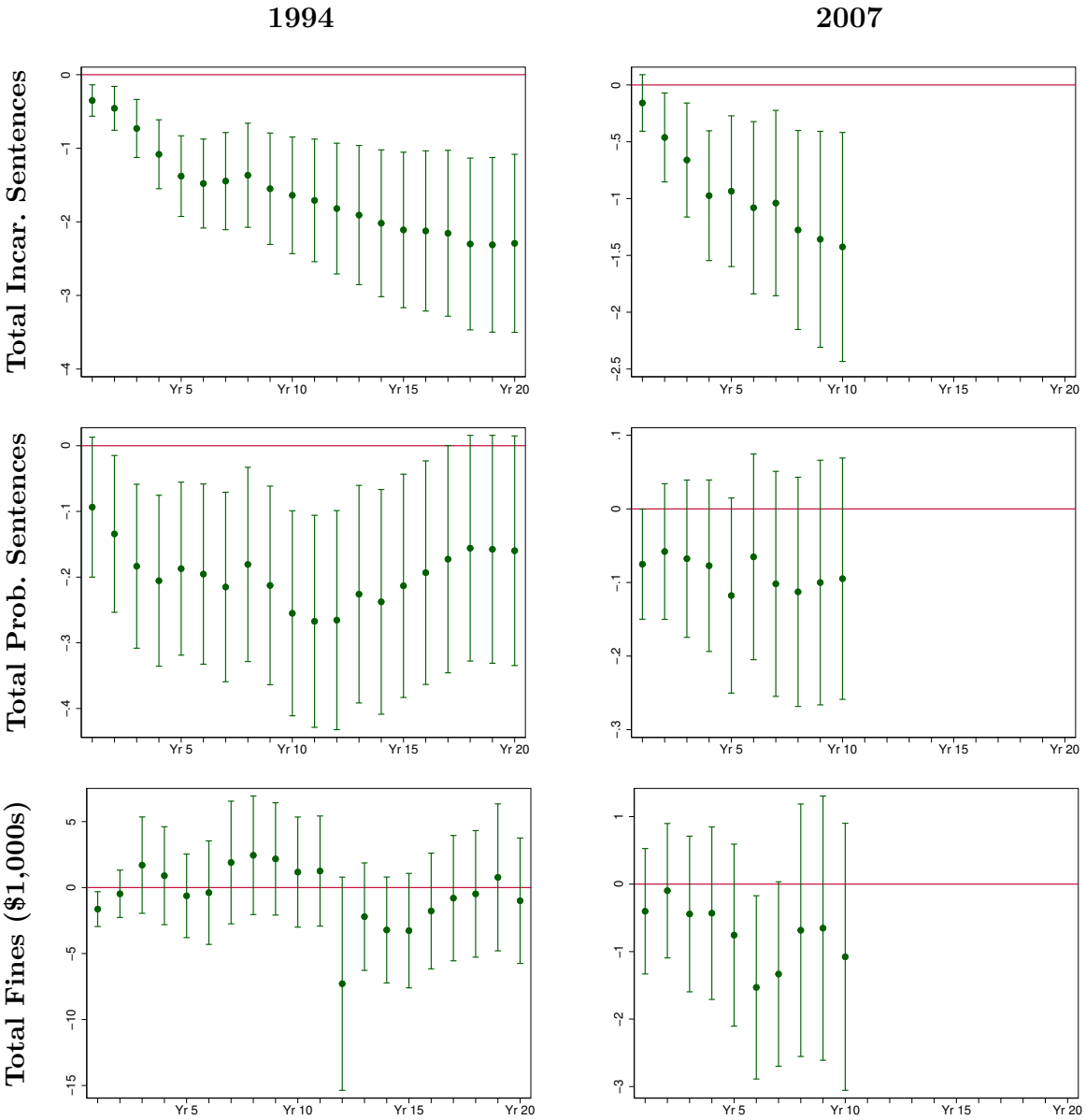


Figure A.3: Accumulation of new sanctions for future criminal activity

This figure displays fuzzy RD estimates and associated 90% confidence bands for incarceration, probation and fine sanctions associated with future criminal activity. The coefficients measure cumulative impacts of diversion for each year period up through 20 years for the 1994 sample (left column) and 10 years for the 2007 sample (right column). The pattern of results are consistent with the amplification effect mechanism described in Section 6. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

A.3 Appendix Tables

Table A.1: Imputation rates in outcome data by follow-up period

Outcome Year	Convictions (1994 Sample)	Earnings (1994 Sample)	Convictions (2007 Sample)	Earnings (2007 Sample)
1	0	0.380	0	0
2	0	0.150	0	0
3	0	0	0	0
4	0	0	0	0
5	0	0	0	0
6	0	0	0	0
7	0	0	0	0
8	0	0	0	0
9	0	0	0.230	0.160
10	0	0	0.480	0.420

This table describes the share of observations by outcome variable, follow-up year and estimation sample that include imputed values based on our methodology described in Section 3.2. We impute data due to the timing of available data. In the 1994 sample, we cannot observe initial labor market outcomes for those charged prior to Jan. 1, 1994; in the 2007 sample, we cannot observe end-of-decade criminal justice and labor market outcomes for those disposed after Oct. 1, 2007 and Jan. 1, 2008 respectively.

Table A.2: Alternative Measures of Recidivism

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
Total Bookings	-1.380**	-1.570*	-1.470***	0.190
	(0.650)	(0.810)	(0.520)	(1.030)
	[2.57]	[2.83]		
Total Charges	-1.640***	-1.600**	-1.620***	-0.039
	(0.530)	(0.700)	(0.440)	(0.880)
	[2.31]	[2.76]		
Total Convictions	-1.610***	-1.290**	-1.450***	-0.320
	(0.480)	(0.630)	(0.400)	(0.790)
	[2.09]	[2.31]		
Total TDPS CCH Conv.	-1.090**	-1.370***	-1.230***	0.280
	(0.480)	(0.400)	(0.310)	(0.620)
	[1.64]	[1.47]		
Observations	31,131	52,792		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the effect of diversion on alternative reoffending outcomes from the estimated impacts on Harris County convictions reported in the third row. The first two rows report estimates for alternative measures of recidivism available from Harris County: jail bookings and charges filed. The fourth row reports the impact of diversion on the total number of convictions recorded in the statewide CCH conviction database. *General RD Table Notes* from Table 1 apply.

Table A.3: Comparison of Harris and non-Harris County Recidivism in the TDPS CCH

	τ^{94}	τ^{07}	$\frac{\tau^{94}+\tau^{07}}{2}$	$H_0 : \tau^{94} = \tau^{07}$
TDPS CCH Conv. (All Counties)	-1.090**	-1.370***	-1.230***	0.280
	(0.480)	(0.400)	(0.310)	(0.620)
	[1.64]	[1.47]		
TDPS CCH Conv. (Harris County)	-1.070***	-0.990***	-1.030***	-0.083
	(0.410)	(0.350)	(0.270)	(0.540)
	[1.40]	[1.22]		
TDPS CCH Conv. (Non-Harris Counties)	-0.055	-0.490***	-0.270**	0.440*
	(0.190)	(0.170)	(0.130)	(0.260)
	[0.23]	[0.25]		
Observations	31,131	52,792		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents fuzzy RD estimates of the impact of diversion on the total number of convictions recorded in the statewide CCH conviction database in the first row and then separates outcomes into Harris County convictions and non-Harris County convictions to evaluate spatial spillovers. *General RD Table Notes* from Table 1 apply.

Table A.4: Placebos Exercises using Alternate Year Discontinuities

	τ^{93}	τ^{95}	τ^{06}	τ^{08}
Sharp RD: Diversion	-0.031	-0.015	0.034	-0.028
	(0.031)	(0.034)	(0.026)	(0.026)
	[0.65]	[0.35]	[0.44]	[0.56]
Sharp RD: Total Convictions	-0.053	-0.012	-0.059	-0.017
	(0.120)	(0.150)	(0.140)	(0.100)
	[1.23]	[1.47]	[1.47]	[1.12]
Sharp RD: Qtrly Employment Rate	-0.065**	-0.024	0.008	-0.020
	(0.031)	(0.027)	(0.019)	(0.022)
	[0.40]	[0.41]	[0.36]	[0.39]
Observations	15,430	15,942	26,240	26,606

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents sharp RD estimates of the impact of the discontinuity at four placebo threshold dates on diversion, total convictions and the average quarterly employment rate. We shift the true discontinuity dates back one year in the first and third column to 9/1/1993 and 11/7/2006, respectively; and forward one year in the second and fourth column to 9/1/1995 and 11/7/2008, respectively. We do not report fuzzy RD estimates since there is zero first-stage relationship. *General RD Table Notes* from Table 1 apply.

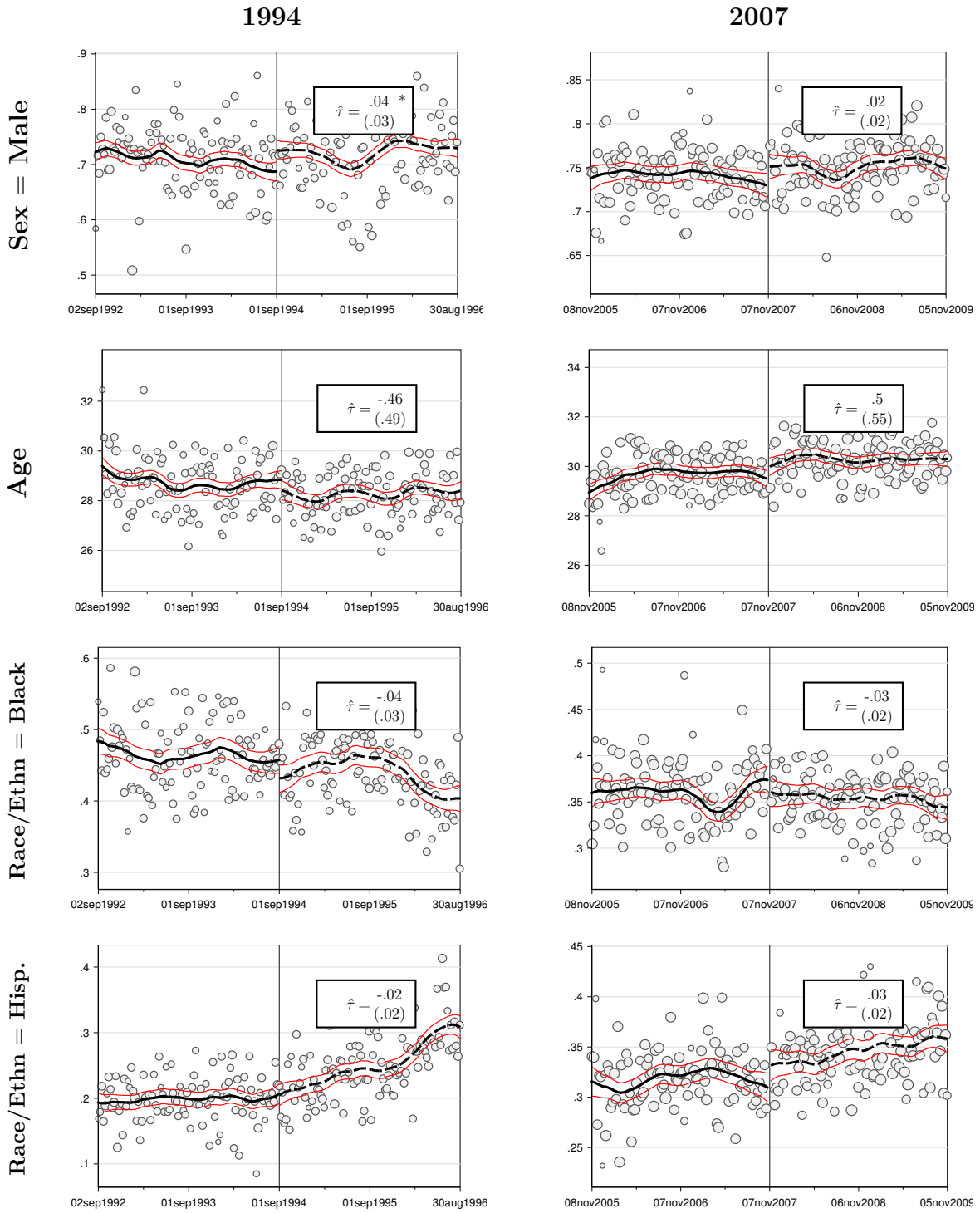
Table A.5: Comparison between First Time Felony Defendants and Repeat Felony Defendants

	1994 First Time	1994 Repeat	2007 First Time	2007 Repeat
Sharp RD: Diversion	-0.240***	-0.006	0.180***	0.120***
	(0.028)	(0.034)	(0.025)	(0.025)
	[0.69]	[0.29]	[0.49]	[0.12]
Fuzzy RD: Total Convictions	-1.610***		-1.210*	0.310
	(0.480)		(0.630)	(2.180)
	[2.21]		[2.16]	[3.88]
Fuzzy RD: Qtrly Employment Rate	0.200***		0.160	0.100
	(0.078)		(0.100)	(0.140)
	[0.37]		[0.30]	[0.15]
Observations	31,334	15,780	52,846	26,865

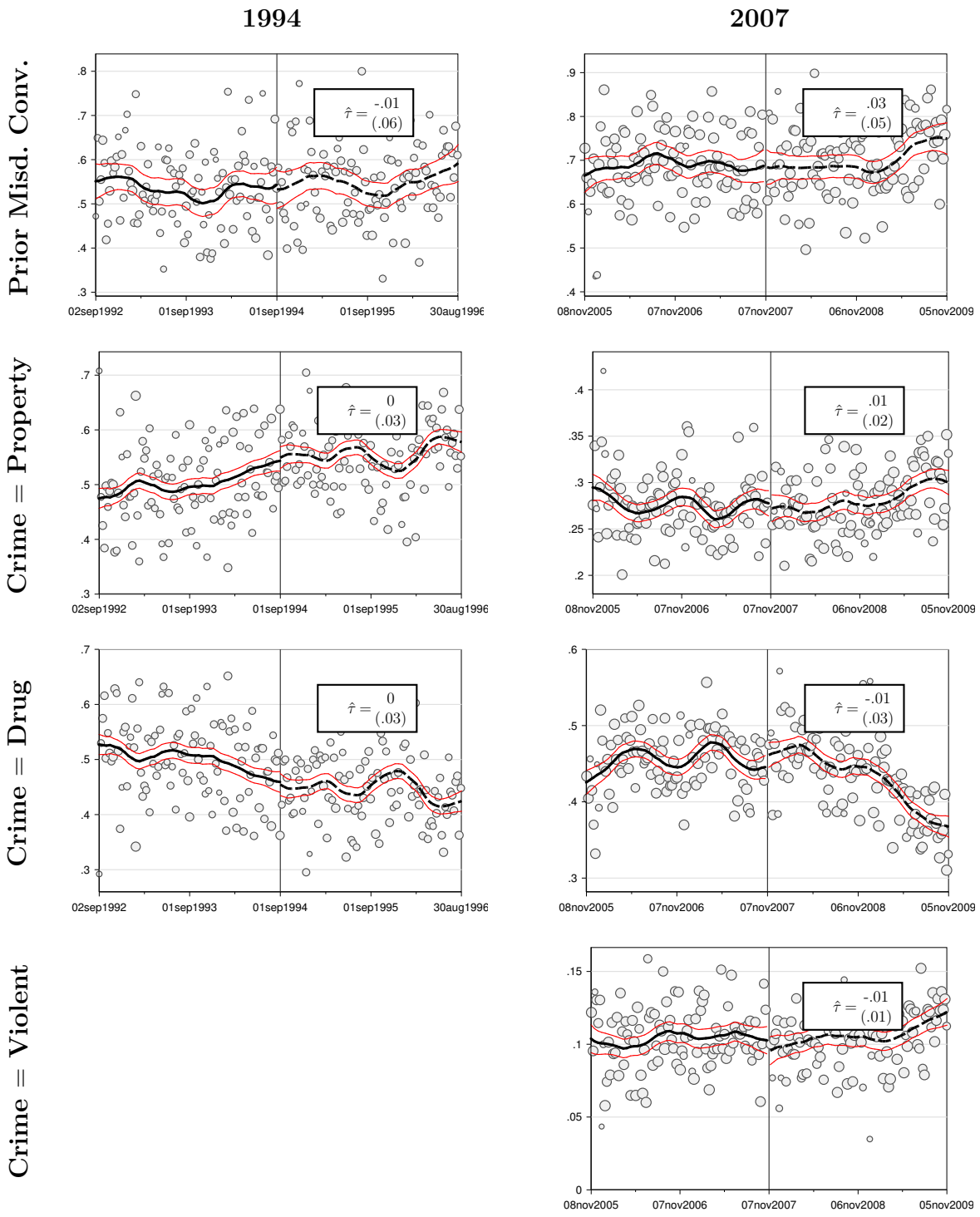
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table compares RD estimates for our estimation sample of first-time felony defendants with samples of individuals with prior felony convictions, but who meet all other sample requirements, for each experiment. Sharp RD estimates are reported for all groups in the first row. We then report fuzzy RD estimates for our primary reoffending and employment outcomes in the second and third rows. We do not report the second-stage fuzzy RD estimates for the repeat offender group in 1994 since effectively there is zero first-stage relationship. *General RD Table Notes* from Table 1 apply.

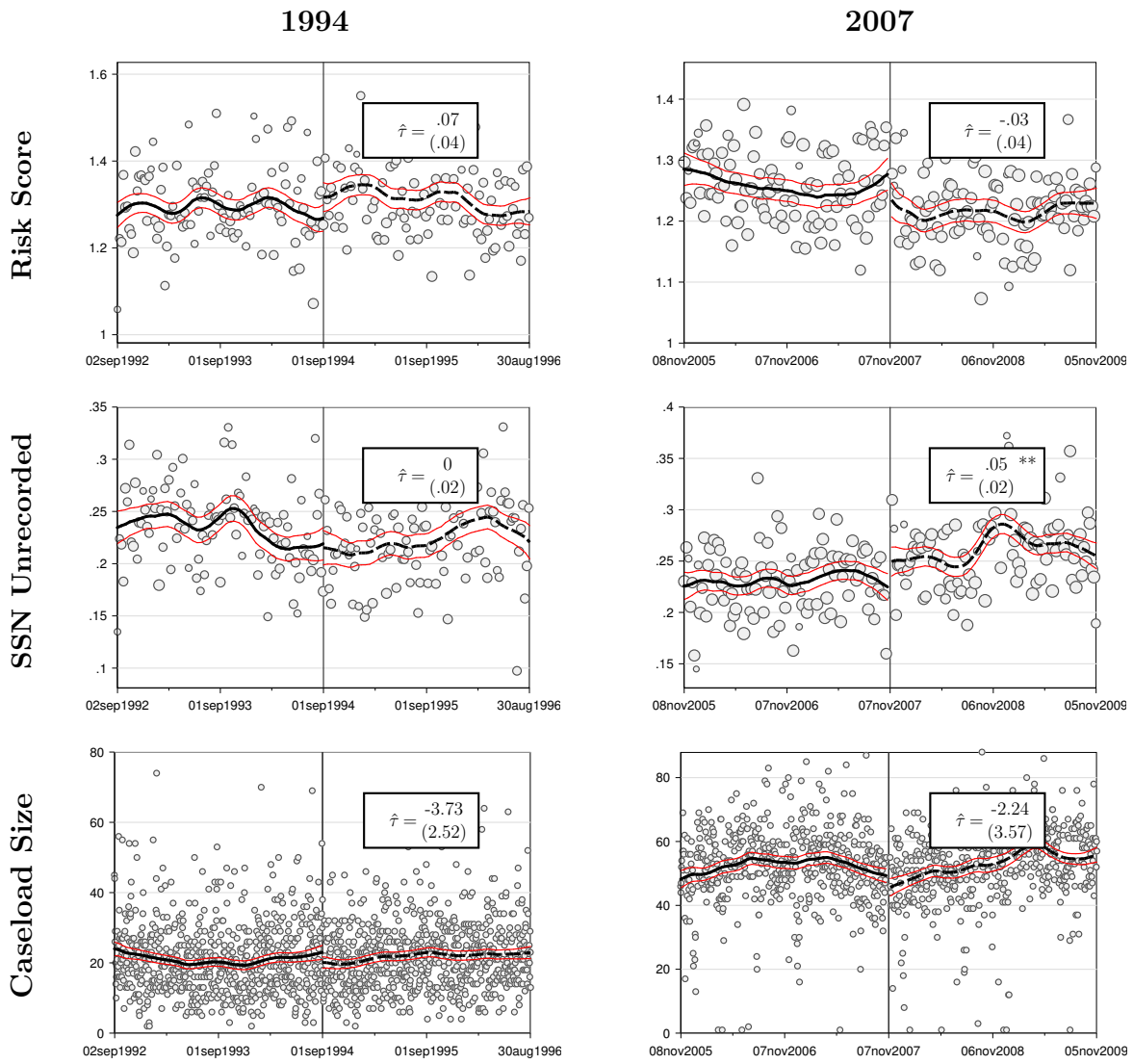
B Online Appendix (Not for Publication)



Online Figure B.1: Baseline comparison

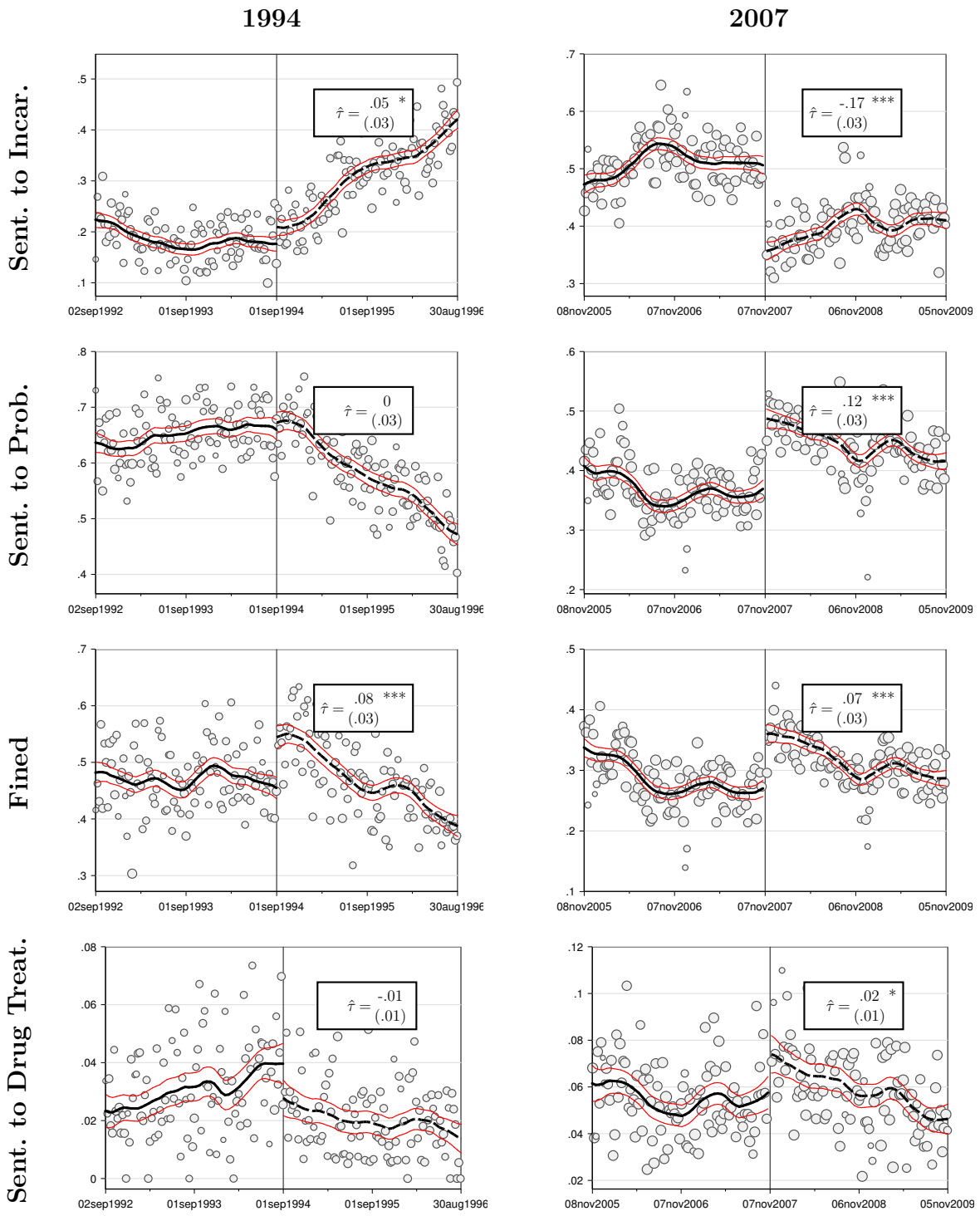


Online Figure B.1: Baseline comparison (continued)



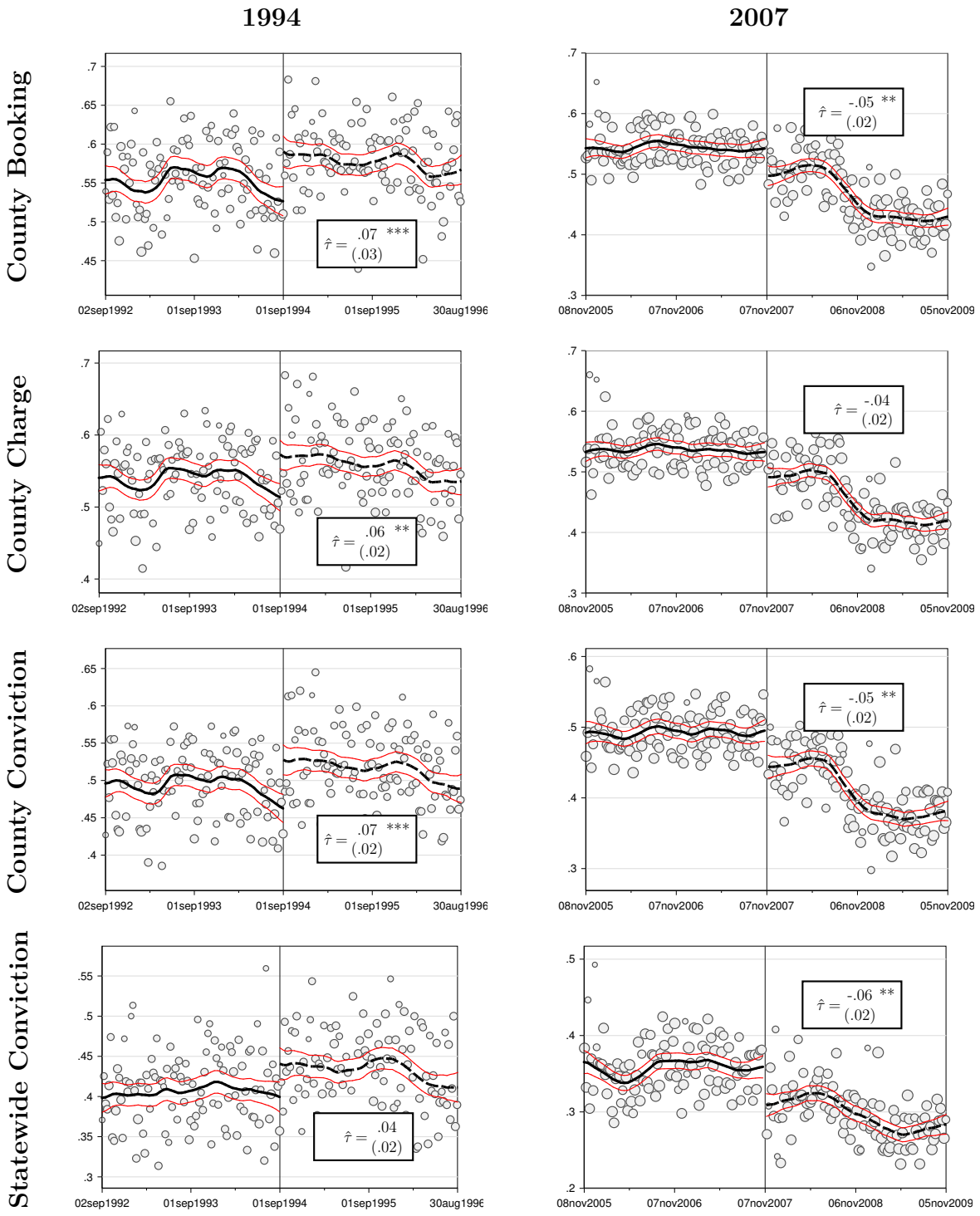
Online Figure B.1: Baseline comparison (continued)

This figure presents reduced form graphical evidence of any changes in pre-determined characteristics as recorded in the criminal court records from the Harris County District Court associated with the 1994 (left column) and 2007 (right column) natural experiments. The calculation of the recidivism risk score is described in Section 5. SSN Unrecorded is an indicator variable for there being no recorded social security number in the court record. These estimates correspond to coefficients presented in Table 1. All *General RD Figure Notes* from Figure 1 apply.



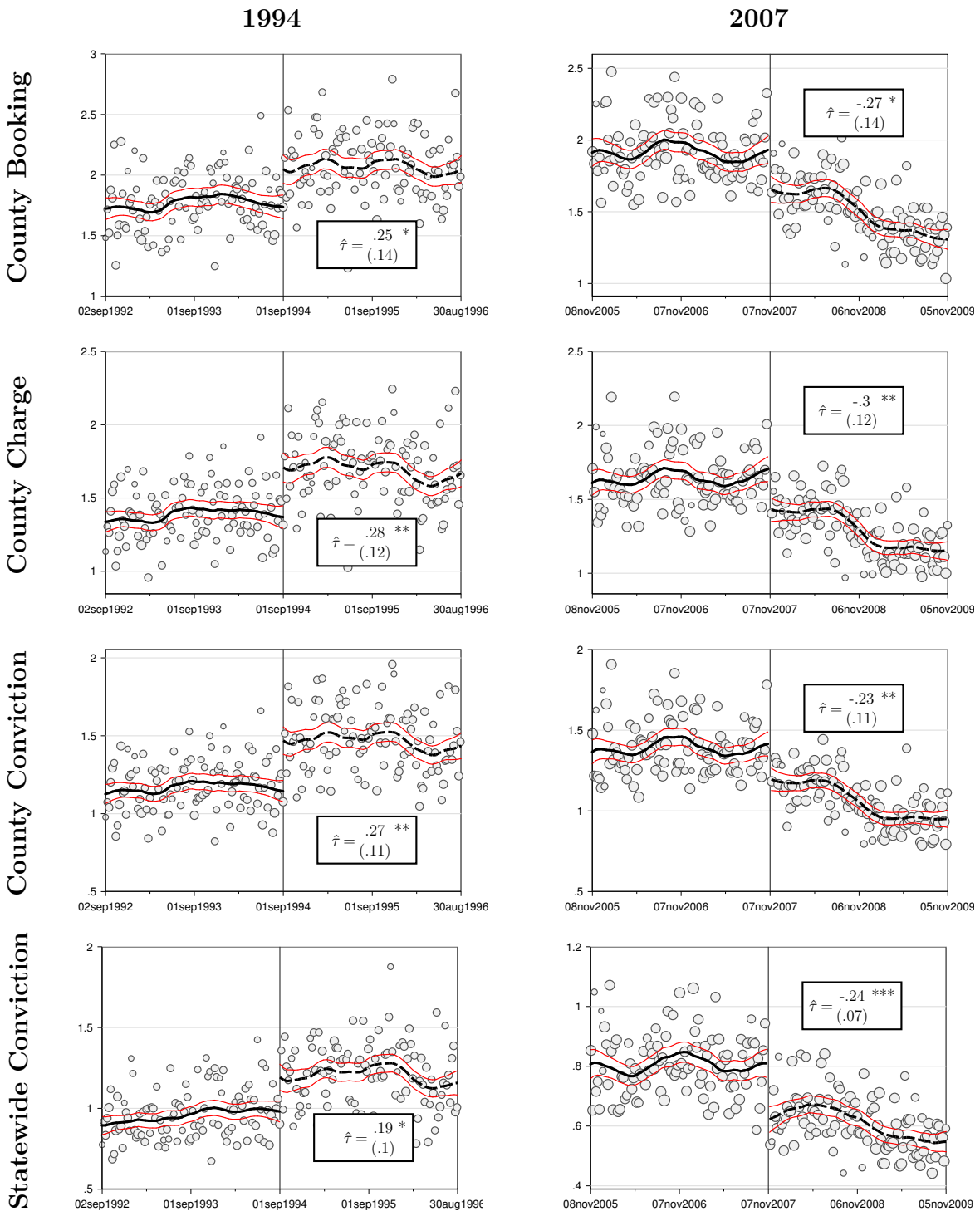
Online Figure B.2: Visual Evidence and Estimated Reduced Form Change - Sentencing Outcomes

This figure presents reduced form graphical evidence of any changes in sentencing outcomes recorded in the criminal court records from the Harris County District Court associated with the 1994 (left column) and 2007 (right column) natural experiments. These estimates represent the reduced form version of the fuzzy RD estimates presented in Table 2. All *General RD Figure Notes* from Figure 1 apply.



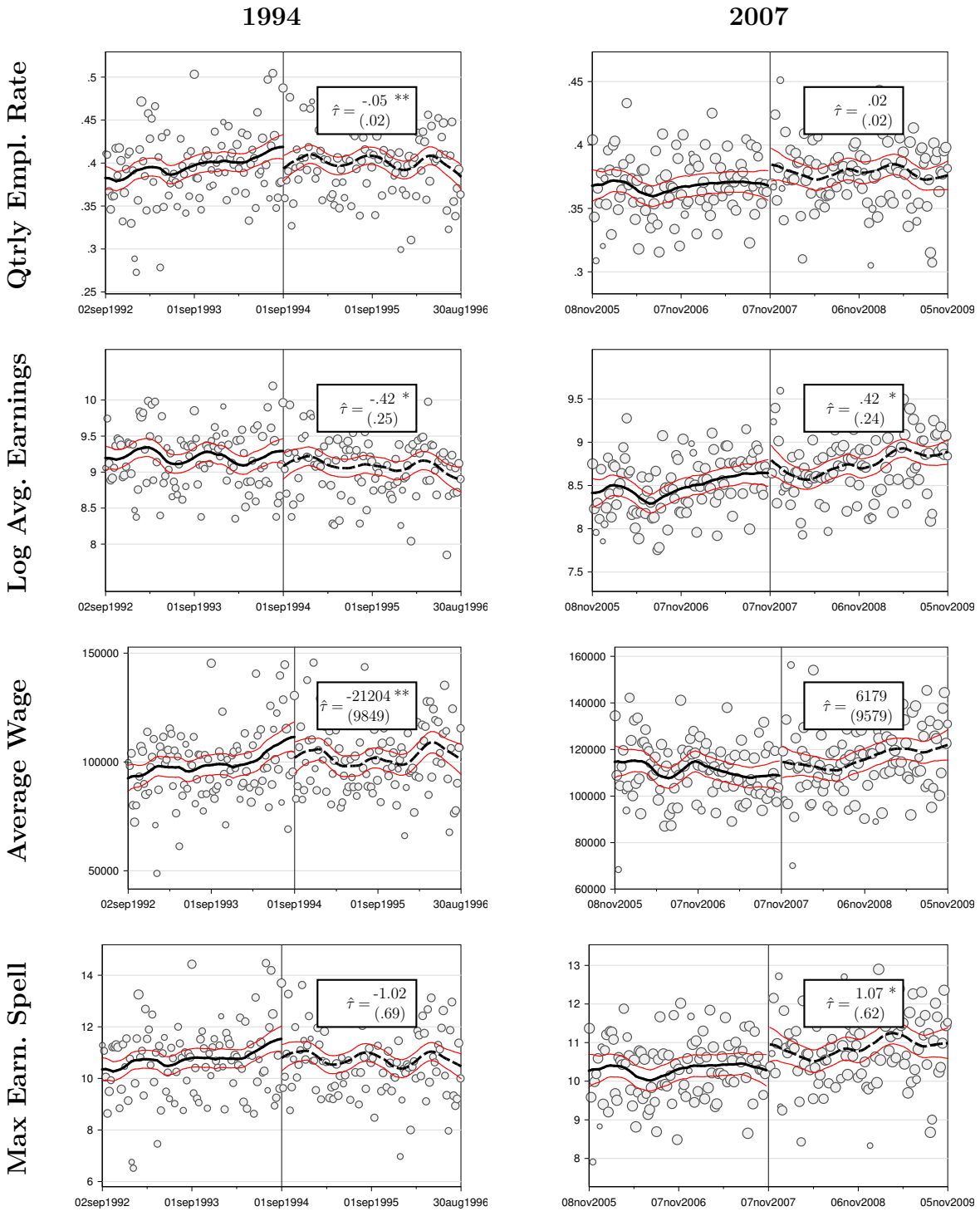
Online Figure B.3: Visual Evidence and Estimated Reduced Form Change - Recidivism (Extensive Margin)

This figure presents reduced form graphical evidence of any changes in reoffending outcomes at the extensive-margin associated with the 1994 (left column) and 2007 (right column) natural experiments. All *General RD Figure Notes* from Figure 1 apply.



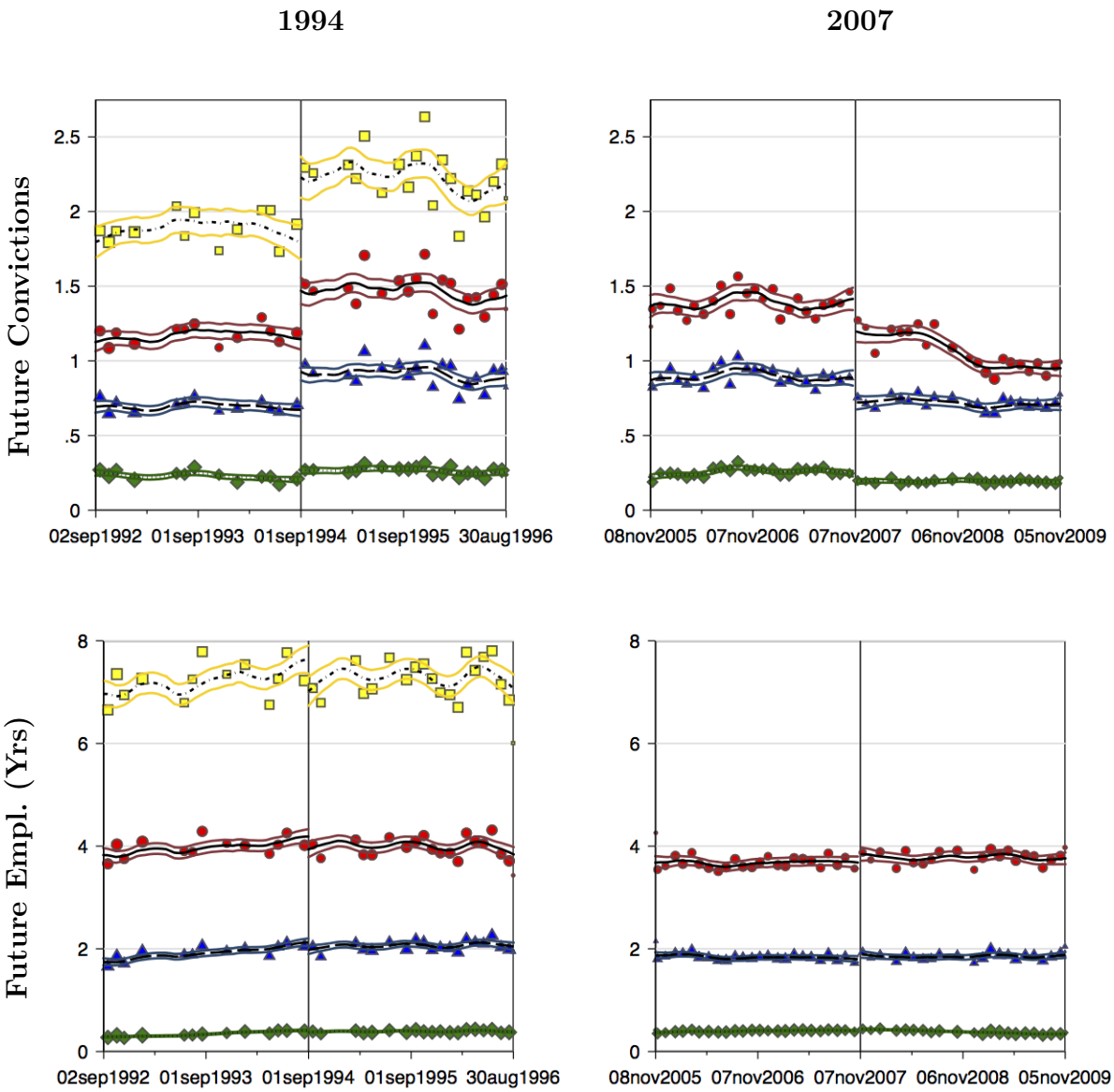
Online Figure B.4: Visual Evidence and Estimated Reduced Form Change - Recidivism (Intensive Margin)

This figure presents reduced form graphical evidence of any changes in reoffending outcomes at the intensive-margin associated with the 1994 (left column) and 2007 (right column) natural experiments. These results are reduced-form versions of the fuzzy RD reoffending results presented in Table A.2. All *General RD Figure Notes* from Figure 1 apply.



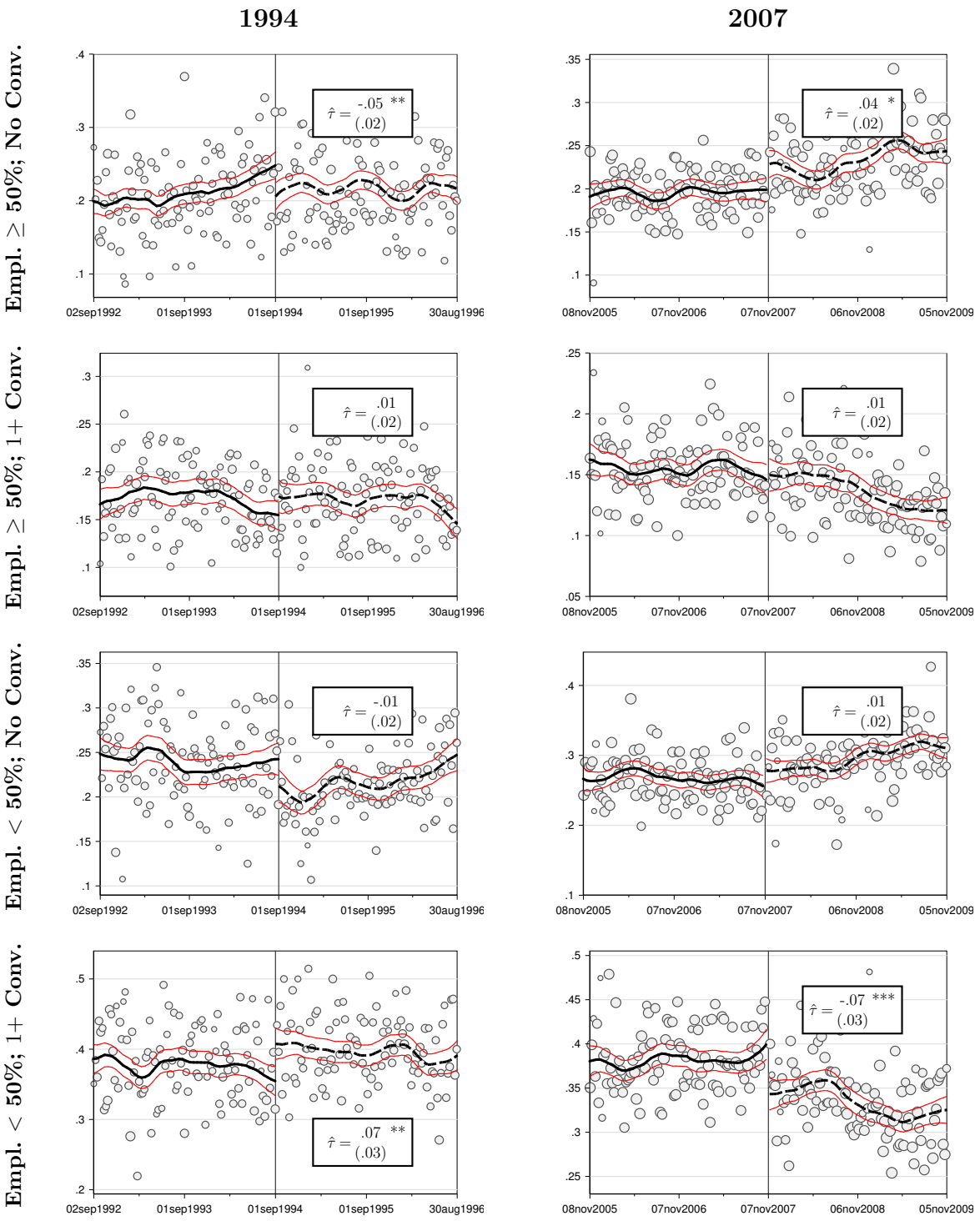
Online Figure B.5: Visual Evidence and Estimated Reduced Form Change - Labor Market

This figure presents reduced form graphical evidence of any changes in labor market outcomes with the 1994 (left column) and 2007 (right column) natural experiments. These results are reduced-form versions of the fuzzy RD reoffending results presented in Table 4. All *General RD Figure Notes* from Figure 1 apply.



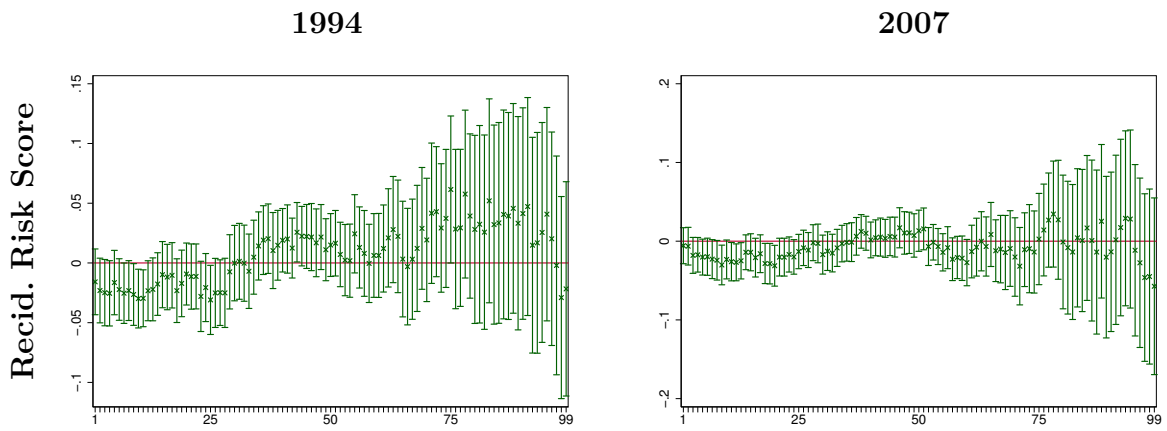
Online Figure B.6: Visual Evidence on the 1, 5, 10 and 20-year Cumulative Impacts

This figure presents reduced form graphical evidence on the cumulative effect of diversion on future convictions and employment associated with the 1994 (left column) and 2007 (right column) natural experiments. These estimates represent the reduced form version of the fuzzy RD estimates presented in Figure 2. To distinguish the different follow-up periods, we have used the following schema: 1 year (dotted regression line, green CI, diamond scatter), 5 year (dashed regression line, blue CI, triangle scatter), 10 year (solid regression line, red CI, circle scatter), and 20 year (dotted-dashed regression line, gold CI, square scatter). For readability, the scatter plots show monthly rather than weekly bins. Otherwise, all *General RD Figure Notes* from Figure 1 apply.



Online Figure B.7: Visual Evidence and Estimated Reduced Form Change - Combined Crime/Labor Outcomes

This figure presents reduced form graphical evidence of any changes in the offending/labor market intersection outcomes (defined in Table 5) associated with the 1994 (left column) and 2007 (right column) natural experiments. These results are reduced-form versions of the fuzzy RD results presented in Table 5. All *General RD Figure Notes* from Figure 1 apply.



Online Figure B.8: Balance of Predicted Recidivism Risk over the Recidivism Risk Profile

This figure displays RD estimates and associated 90% confidence bands for evaluating balance in predicted recidivism risk score described in Section 5.4 calculated over the quantile function of the predicted recidivism risk score for the 1994 experiment (left column) and 2007 experiment (right column). Each coefficient reflects a distinct local polynomial RD estimate from regression centered at the focal percentile using a uniform kernel with a 40 percentile bandwidth. Estimates below the 20th and above the 80th percentiles will reflect narrower, asymmetric bandwidths. RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2. We do not adjust the regression estimates for observable covariates because this is a balance test.

Online Table B.1: Classic Heterogeneity Analysis

Panel A: 1994 Sample	Black	White	Hispanic	Crime = Property	Crime = Drug	
Sharp RD: Diversion	-0.310***	-0.250***	-0.160***	-0.150***	-0.370***	
	(0.044)	(0.045)	(0.055)	(0.047)	(0.043)	
Fuzzy RD: Total Convictions	-1.270**	-0.930	-1.070	-1.990**	-0.630	
	(0.630)	(0.640)	(1.190)	(0.820)	(0.480)	
Fuzzy RD: Qtrly Employment Rate	0.230**	0.270*	-0.200	0.360**	-0.004	
	(0.110)	(0.150)	(0.260)	(0.160)	(0.068)	
Observations	12,456	7,035	4,313	12,433	11,696	
	Male	Female	< 30 Yrs Old	≥ 30 Yrs Old	No Misd. Conv.	1+ Misd. Conv.
Sharp RD: Diversion	-0.260***	-0.250***	-0.230***	-0.290***	-0.330***	-0.230***
	(0.034)	(0.052)	(0.034)	(0.053)	(0.042)	(0.038)
Fuzzy RD: Total Convictions	-1.040*	-1.000	-1.720**	-0.700*	-0.490	-1.810***
	(0.610)	(0.760)	(0.750)	(0.410)	(0.590)	(0.570)
Fuzzy RD: Qtrly Employment Rate	0.038	0.310	0.140	0.033	-0.014	0.250**
	(0.073)	(0.200)	(0.088)	(0.120)	(0.110)	(0.120)
Observations	22,419	8,915	18,477	12,857	8,988	22,346

Panel B: 2007 Sample	Black	White	Hispanic	Crime = Property	Crime = Drug	Crime = Violent
Sharp RD: Diversion	0.190***	0.170***	0.130***	0.088**	0.250***	0.180***
	(0.039)	(0.046)	(0.047)	(0.042)	(0.044)	(0.060)
Fuzzy RD: Total Convictions	-2.060**	-1.000	-1.570*	-2.740	-0.370	-1.160
	(0.880)	(1.020)	(0.910)	(1.800)	(0.630)	(1.400)
Fuzzy RD: Qtrly Employment Rate	0.210	0.100	0.190	0.024	0.140*	0.160
	(0.130)	(0.170)	(0.180)	(0.340)	(0.088)	(0.490)
Observations	16,804	11,757	10,683	11,287	17,267	4,261
	Male	Female	< 30 Yrs Old	≥ 30 Yrs Old	No Misd. Conv.	1+ Misd. Conv.
Sharp RD: Diversion	0.170***	0.160***	0.160***	0.200***	0.190***	0.150***
	(0.033)	(0.053)	(0.035)	(0.038)	(0.040)	(0.038)
Fuzzy RD: Total Convictions	-1.730**	-1.000	-2.380***	-0.390	-1.370	-1.270
	(0.700)	(1.210)	(0.780)	(0.770)	(0.980)	(0.800)
Fuzzy RD: Qtrly Employment Rate	0.150	0.048	0.093	0.110	0.082	0.150
	(0.110)	(0.170)	(0.120)	(0.130)	(0.110)	(0.160)
Observations	39,464	13,382	30,513	22,333	18,012	34,834

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents estimates of the impact of diversion for subgroups defined based on demographic characteristics or prior criminal histories as indicated in the title to each column. Panel A presents estimates across our key first stage (diversion) and second stage (total convictions and quarterly employment rate) for 11 subsamples. Panel B repeats this exercise for the 2007 experiment. *General RD Table Notes* from Table 1 apply.

Online Table B.2: Robustness - Bandwidth

	MSE1	MSE2	MSE3	CER1	CER2	CER3
Panel A: 1994 Sample						
Sharp RD: Diversion	-0.260***	-0.240***	-0.230***	-0.240***	-0.220***	-0.210***
	(0.027)	(0.028)	(0.029)	(0.031)	(0.033)	(0.034)
Fuzzy RD: Total Convictions	-1.700***	-1.490***	-1.610***	-1.610***	-1.790***	-1.630***
	(0.490)	(0.500)	(0.480)	(0.590)	(0.590)	(0.580)
Fuzzy RD: Qtrly Employment Rate	0.180**	0.200***	0.054	0.140	0.180*	0.140**
	(0.083)	(0.078)	(0.055)	(0.110)	(0.100)	(0.069)
Panel B: 2007 Sample						
Sharp RD: Diversion	0.170***	0.180***	0.170***	0.160***	0.160***	0.180***
	(0.025)	(0.026)	(0.019)	(0.031)	(0.031)	(0.024)
Fuzzy RD: Total Convictions	-1.160*	-1.210*	-1.370**	-0.810	-0.720	-0.790
	(0.630)	(0.650)	(0.630)	(0.910)	(0.930)	(0.950)
Fuzzy RD: Qtrly Employment Rate	0.160	0.160	0.200*	0.130	0.110	0.120
	(0.100)	(0.100)	(0.110)	(0.150)	(0.150)	(0.160)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents sharp RD (diversion) and fuzzy RD (total convictions and employment rate) estimates varying the optimal bandwidth selection method. The bandwidth selectors are coded as follows: MSE1 (one common mean squared error-optimal bandwidth selector for the RD treatment effect estimator); MSE2 (two different mean squared error-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); MSE3 (one common mean squared error-optimal bandwidth selector for the sum of regression estimates); CER1 (one common coverage error rate-optimal bandwidth selector for the RD treatment effect estimator); CER2 (two different coverage error rate-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator); and, CER3 (one common coverage error rate-optimal bandwidth selector for the sum of regression estimates). All other RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Online Table B.3: Robustness - Variance Estimators

	Day Cluster	Week Cluster	Nearest Neighbor
Panel A: 1994 Sample			
Sharp RD: Diversion	-0.240***	-0.240***	-0.240***
	(0.033)	(0.032)	(0.028)
Fuzzy RD: Total Convictions	-1.340**	-1.520***	-1.610***
	(0.530)	(0.500)	(0.480)
Fuzzy RD: Qtrly Employment Rate	0.160*	0.160	0.220***
	(0.088)	(0.110)	(0.080)
Panel B: 2007 Sample			
Sharp RD: Diversion	0.170***	0.170***	0.180***
	(0.032)	(0.027)	(0.025)
Fuzzy RD: Total Convictions	-1.430**	-1.040*	-1.210*
	(0.560)	(0.580)	(0.630)
Fuzzy RD: Qtrly Employment Rate	0.180*	0.210**	0.160
	(0.097)	(0.084)	(0.100)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents sharp RD (diversion) and fuzzy RD (total convictions and employment rate) estimates using alternative variance estimators as described with column titles. All other RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.

Online Table B.4: Robustness - Alternative Specificaitons

	No Donut	No Covariates	Non-Robust	Non-Bias Corrected	Week Binned Running Var.	Epanechnikov Kernel	Triangular Kernel
Panel A: 1994 Sample							
Sharp RD: Diversion	-0.240***	-0.240***	-0.240***	-0.250***	-0.190***	-0.260***	-0.250***
	(0.028)	(0.029)	(0.025)	(0.025)	(0.028)	(0.025)	(0.025)
Fuzzy RD: Total Convictions	-1.610***	-1.440***	-1.610***	-1.350***	-1.880***	-1.210***	-1.350***
	(0.480)	(0.530)	(0.420)	(0.420)	(0.600)	(0.390)	(0.420)
Fuzzy RD: Qtrly Employment Rate	0.200***	0.170**	0.200***	0.180***	0.062	0.180**	0.180**
	(0.078)	(0.077)	(0.069)	(0.069)	(0.082)	(0.076)	(0.078)
Panel B: 2007 Sample							
Sharp RD: Diversion	0.130***	0.170***	0.180***	0.180***	0.170***	0.170***	0.170***
	(0.023)	(0.025)	(0.021)	(0.021)	(0.024)	(0.025)	(0.024)
Fuzzy RD: Total Convictions	-1.560**	-1.690**	-1.210**	-1.140**	-1.440**	-1.300**	-1.310**
	(0.700)	(0.700)	(0.530)	(0.530)	(0.630)	(0.610)	(0.650)
Fuzzy RD: Qtrly Employment Rate	0.100	0.140	0.160*	0.140	0.160	0.150	0.160
	(0.110)	(0.110)	(0.089)	(0.089)	(0.100)	(0.100)	(0.100)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This table presents sharp RD (diversion) and fuzzy RD (total convictions and employment rate) estimates that relax other baseline specification choices that are described with column titles. All other RD specification choices are described in Section 4 and the estimation sample for each experiment is described in Section 3.2.